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THE CULT OF 'COMMON USAGE' *

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THE most influential school of philosophy in Britain at the present day maintains a certain linguistic doctrine to which I am unable to subscribe. I do not wish to misrepresent this school, but I suppose any opponent of any doctrine is thought to misrepresent it by those who hold it. The doctrine, as I understand it, consists in maintaining that the language of daily life, with words used in their ordinary meanings, suffices for philosophy, which has no need of technical terms or of changes in the signification of common terms. I find myself totally unable to accept this view. I object to it because :

- (1) It is insincere ;
- (2) It is capable of excusing ignorance of mathematics, physics, and neurology in those who have had only a classical education ;
- (3) It is advanced by some in a tone of unctuous rectitude, as if opposition to it were a sin against democracy ;
- (4) It makes philosophy trivial ;
- (5) It makes almost inevitable the perpetuation among philosophers of the muddle-headedness they have taken over from common sense.

(1) *Insincerity.* I will illustrate this by a fable. The professor of Mental Philosophy, when called by his bedmaker one morning, developed a dangerous frenzy, and had to be taken away by the police in an ambulance. I heard a colleague, a believer in 'common usage', asking the poor philosopher's doctor about the occurrence. The doctor replied that the professor had had an attack of temporary psychotic instability, which had subsided after an hour. The believer in 'common usage', so far from objecting to the doctor's language,

* Received 7. xi. 52

repeated it to other enquirers. But it happened that I, who live on the professor's staircase, overheard the following dialogue between the bedmaker and the policeman :

Policeman : 'Ere, I want a word with yer.

Bedmaker : What do you mean ? 'A word' ? I ain't done nothing.

Policeman : Ah, that's just it—Yer ought to 'ave done something. Couldn't yer see the pore gentleman was mental ?

Bedmaker : That I could. For an 'ole hour 'e went on something chronic. But when they're mental you can't make them understand.

In this little dialogue, 'word', 'mean', 'mental', and 'chronic' are all used in accordance with common usage. They are not so used in the pages of *Mind* by those who pretend that common usage is what they believe in. What in fact they believe in is not common usage, as determined by mass observation, statistics, medians, standard deviations, and the rest of the apparatus. What they believe in is the usage of persons who have their amount of education, neither more nor less—less is illiteracy, more is pedantry—so we are given to understand.

(2) *An excuse for ignorance.* Every motorist is accustomed to speedometers and accelerators, but unless he has learnt mathematics he attaches no precise significance to 'speed' or 'acceleration'. If he does attach a precise significance to these words, he will know that his speed and his acceleration are at every moment unknowable, and that, if he is fined for speeding, the conviction must be based on insufficient evidence if the time when he is supposed to have speeded is mentioned. On these grounds I will agree with the advocate of common usage that such a word as 'speed', if used in daily life, must be used as in daily life, and not as in mathematics. But then it should be realised that 'speed' is a vague notion, and that equal truth may attach to all three of the statements in the conjugation of the following irregular verb :

'I was at rest' (motorist),

'You were moving at 20 miles an hour' (a friend),

'He was travelling at 60 miles an hour' (the police).

It is because this state of affairs is puzzling to magistrates that mathematicians have abandoned common usage.

(3) Those who advocate common usage in philosophy sometimes speak in a manner that suggests the *mystique* of the 'common man'. They may admit that in organic chemistry there is need of long

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words, and that quantum physics requires formulae that are difficult to translate into ordinary English, but philosophy (they think) is different. It is not the function of philosophy—so they maintain—to teach something that uneducated people do not know; on the contrary, its function is to teach superior persons that they are not as superior as they thought they were, and that those who are *really* superior can show their skill by making sense of common sense.

It is, of course, a dreadful thing in these days to lay claim to any kind of superiority except in athletics, movies, and money-making. Nevertheless I will venture to say that in former centuries common sense made what we now think mistakes. It used to be thought that there could not be people at the antipodes, because they would fall off, or, if they avoided that, they would grow dizzy from standing on their heads. It used to be thought absurd to say that the earth rotates because everybody can see that it doesn't. When it was first suggested that the sun may be as large as the Peloponnesus, common sense was outraged; but all this was long ago. I do not know at what date common sense became all-wise. Perhaps it was in 1776; perhaps in 1848; or perhaps with the passing of the Education Act in 1870. Or perhaps it was only when physiologists such as Adrian and Sherrington began to make scientific inroads on philosophers' ideas about perception.

(4) Philosophy, as conceived by the school I am discussing, seems to me a trivial and uninteresting pursuit. To discuss endlessly what silly people mean when they say silly things may be amusing but can hardly be important. Does the full moon look as large as half a crown or as large as a soup plate? Either answer can be proved correct by experiment. It follows that there is an ambiguity in the question. A modern philosopher will clear up the ambiguity for you with meticulous care.

But let us take an example which is less unfair, say the question of immortality. Orthodox Christianity asserts that we survive death. What does it mean by this assertion? And in what sense, if any, is the assertion true? The philosophers with whom I am concerned will consider the first of these questions, but will say that the second is none of their business. I agree entirely that, in this case, a discussion as to what is meant is important and highly necessary as a preliminary to a consideration of the substantial question, but if nothing can be said on the substantial question it seems a waste of time to discuss what it means. These philosophers remind me of

the shopkeeper of whom I once asked the shortest way to Winchester. He called to a man in the back premises :

- ' Gentleman wants to know the shortest way to Winchester.'
- ' Winchester ? ' an unseen voice replied.
- ' Aye.'
- ' Way to Winchester ? '
- ' Aye.'
- ' Shortest way ? '
- ' Aye.'
- ' Dunno.'

He wanted to get the nature of the question clear, but took no interest in answering it. This is exactly what modern philosophy does for the earnest seeker after truth. Is it surprising that young people turn to other studies ?

(5) Common sense, though all very well for everyday purposes, is easily confused, even by such simple questions as ' Where is the rainbow ? ' When you hear a voice on a gramophone record, are you hearing the man who spoke, or a reproduction ? When you feel a pain in a leg that has been amputated, where is the pain ? If you say it is in your head, would it be in your head if the leg had not been amputated ? If you say yes, then what reason have you ever for thinking you have a leg ? And so on.

No one wants to alter the language of common sense, any more than we wish to give up talking of the sun rising and setting. But astronomers find a different language better, and I contend that a different language is better in philosophy.

Let us take an example. A philosophy containing such a large linguistic element cannot object to the question : What is meant by the word ' word ' ? But I do not see how this is to be answered within the vocabulary of common sense. Let us take the word ' cat ', and for the sake of definiteness let us take the written word. Clearly there are many instances of the word, no one of which is the word. If I say ' Let us discuss the word " cat " ', the word ' cat ' does not occur in what I say, but only an instance of the word. The word itself is no part of the sensible world ; if it is anything, it is an eternal super-sensible entity in a Platonic heaven. The word, we may say, is a class of similar shapes, and, like all classes, is a logical fiction.

But our difficulties are not at an end. Similarity is neither necessary nor sufficient to make a shape a member of the class which

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is the word 'cat'. The word may be written in capitals or in small letters, legibly or illegibly, in black on a white ground or in white on a blackboard. If I write the word 'catastrophe', the first three letters do not constitute an instance of the word 'cat'. The most necessary thing in an instance of the word is *intention*. If a piece of marble happened to have a vein making the shape 'cat' we should not think this an instance of the word.

It thus appears that we cannot define the word 'word' without (a) a logical theory of classes, and (b) a psychological understanding of intention. These are difficult matters. I conclude that common sense, whether correct or incorrect in the use of words, does not know in the least what words are. I wish I could believe that this conclusion would render it speechless.

Let us take another problem, that of perception. There is here an admixture of philosophical and scientific questions, but this admixture is inevitable in many questions, or, if not inevitable, can only be avoided by confining ourselves to comparatively unimportant aspects of the matter in hand.

Here is a series of questions and answers.

Q. When I see a table, will what I see be still there if I shut my eyes?

A. That depends upon the sense in which you use the word 'see'.

Q. What is still there when I shut my eyes?

A. This is an empirical question; don't bother me with it, but ask the physicists.

Q. What exists when my eyes are open, but not when they are shut?

A. This again is empirical, but in deference to previous philosophers I will answer you: coloured surfaces.

Q. May I infer that there are two senses of 'see'? In the first, when I 'see' a table, I 'see' something conjectural about which physics has vague notions that are probably wrong. In the second, I 'see' coloured surfaces which cease to exist when I shut my eyes.

A. That is correct if you want to think clearly, but our philosophy makes clear thinking unnecessary. By oscillating between the two meanings, we avoid paradox and shock, which is more than most philosophers do.

A NOTE ON COMPARATIVE INDUCTIVE LOGIC*

YEHOSHUA BAR-HILLEL

IN § 86 of *Logical Foundations of Probability* (University of Chicago Press, 1950), Carnap deals with the classificatory semantical concept of confirmation, the *concept of confirming evidence*. There he shows that it is easy to define an adequate explicatum for this concept on the basis of a given adequate explicatum for the quantitative concept of confirmation. He proceeds to discuss the question whether an adequate explicatum can be defined directly in terms of L-concepts, without the detour through the quantitative concept. This would benefit those logicians and scientists who are sceptical with regard to the possibility of constructing an adequate quantitative inductive logic. And, indeed, Carnap succeeds in defining in the mentioned way a concept $\mathfrak{C}'(h, i, e)$ such that a hypothesis h is $\mathfrak{C}'$ -confirmed by a new observational result i on the basis of the old evidence e if and only if, for every regular c -function, the degree of confirmation c which h has on the basis of e and i together is greater than the degree of confirmation c which h has on the basis of e alone.

But Carnap is able to show that $\mathfrak{C}'$ has only a low degree of adequacy, being clearly too narrow, i.e. there are cases for which there is practical agreement that a certain hypothesis is confirmed by a new result whereas $\mathfrak{C}'$ will not hold. And, indeed, the definiendum of $\mathfrak{C}'$ says roughly that, in addition to other relations, the relation of L-implication should hold in at least one direction between the hypothesis and the new result with respect to the old evidence. This is certainly a strong requirement, so strong that it is not fulfilled in very many cases of the commonsense application of the concept of confirmation. A very simple example, given by Carnap, shows this: We certainly would all of us practically agree that the observation of a white swan, i.e. an individual b which is both a swan and white, confirms the hypothesis that all swans are white, but it is very easy to see that neither does 'for all x , if x is a swan then x is white' L-imply nor is it L-implied by ' b is a swan and b is white', with respect to the tautological evidence or any irrelevant evidence whatsoever.

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It seems, therefore, to be at least doubtful whether an adequate explication of the classificatory concept of confirmation can be found in terms of L-concepts alone, and it is anyhow very probable that the definition of such an explicatum, if possible on these lines at all, will have to be rather complicated. This result leaves those logicians, who do not believe that an adequate general explicatum for the quantitative concept of confirmation can be found, with a not very promising obligation.

Against this negative conclusion, Carnap's treatment of the comparative concept of confirmation in §§ 79-85 gives the impression that a more or less adequate explicatum of this concept can be defined in non-quantitative terms, a result which would satisfy many of the scepticists (or pessimists). I think, therefore, that it is important to see that Carnap's proposed explicatum $\mathcal{MC}$ (or $\mathcal{Gr}$, for this matter) suffers from exactly the same defect as $\mathcal{C}'$, namely in being clearly too narrow. It is very easy to verify that none of the deductive relations mentioned in the third condition of the definiens of $\mathcal{MC}$ (p. 436) is satisfied in the example given above. Whereas everybody would agree that the mentioned hypothesis h is better confirmed by the new result i than it was without it (on any irrelevant evidence), $\mathcal{MC}(h, e.i, h, e)$ does not hold (where e is any irrelevant evidence). Carnap's oversight is the more remarkable as he himself stressed very clearly (p. 465) that ' $\mathcal{Gr}(h, e.i, h, e)$ ' is almost synonymous with ' $\mathcal{C}'(h, i, e)$ ' so that it should have been immediate that it might suffer from the same defects.

$\mathcal{MC}$ was *the* comparative concept of confirmation which was in accord (p. 464) with every regular c-function. Its narrowness shows that only such concepts can serve as adequate explicata which are in accord only with a more restricted class of c-functions (p. 467). I cannot see any difference in the magnitude of the difficulties involved in finding such a satisfactory explication for either the classificatory or the comparative concept.

Our result has a curious impact on Carnap's discussion of Koopman's conception (p. 453). On the one hand, it strengthens Koopman's belief that no direct comparative statements can be obtained with the help of a general theory of comparative confirmation. Therefore it weakens the objective background of Carnap's belief to the contrary, namely that such a theory can be constructed on the basis of L-semantics alone, where L-semantics is presumably meant in such a weak sense that it does not include mathematics, even not

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arithmetic (technically speaking, that it does not contain quantification of number variables or equivalent means of expression). On the other hand, Carnap's general approach is considerably strengthened, since it now appears that the construction of an adequate theory of either comparative or classificatory induction is not easier than the construction of a quantitative theory, and that a good way, perhaps the only way, to the former theories leads through quantitative inductive logic.

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ON THE COMPARATIVE CONCEPT OF CONFIRMATION*

RUDOLF CARNAP

1 *The Problem of an Explication of the Comparative Concept*

THE problem consists in finding an explicit definition for a relation $\mathfrak{MC}$ such that

$$(1) \mathfrak{MC}(h, e, h', e')$$

may be interpreted as follows :

- (2) The hypothesis h is confirmed by the evidence e equally strongly or more strongly than h' by e' .

This condition (2) can be expressed in terms of degree of confirmation c as follows :

$$(3) c(h, e) \geq c(h', e').$$

However, we do not take (3) as a definition for (1), because our aim is to find an explication in a non-quantitative form, i.e. not involving such quantitative concepts as c -functions.

On the basis of $\mathfrak{MC}$, we define $\mathfrak{Gr}$ (meaning roughly : 'Greater confirmation') as follows :

$$(4) \mathfrak{Gr}(h, e, h', e') =_{\text{Df}} \mathfrak{MC}(h, e, h', e') \text{ and not } \mathfrak{MC}(h', e', h, e).$$

Then the comparative condition

$$(5) \mathfrak{Gr}(h, e, h', e')$$

corresponds to the following quantitative condition :

$$(6) c(h, e) > c(h', e').$$

I have constructed¹ a definition of $\mathfrak{MC}$ as a tentative explication for (2) and proved² that (in any finite system) the condition (1) holds if and only if

$$(7) \text{ for every regular } c, \quad c(h, e) \geq c(h', e').$$

Now Yehoshua Bar-Hillel³ has shown that my definition is too narrow.

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¹ R. Carnap, *Logical Foundations of Probability*, Chicago, 1950, henceforth referred to by '[Prob.]'. The definition is D81-I, p. 436.

² L. c., T81-I, p. 440

³ Y. Bar-Hillel, 'A note on comparative inductive logic', this volume, pp. 308-10.

Let us consider for a moment the classificatory concept of confirmation :

(8) The additional information i constitutes confirming evidence for the hypothesis h on the basis of the prior evidence e , or, more specifically :

(9) The hypothesis h is confirmed by e and i together more strongly than by e alone.

This clearly corresponds to the quantitative condition :

$$(10) c(h, e \cdot i) > c(h, e).$$

In my book I discussed a possible explicatum $\mathfrak{C}'$ for (8) and (9), defined in a non-quantitative form. I showed¹ that $\mathfrak{C}'(h, i, e)$ if and only if

$$(11) \text{ for every regular } c, c(h, e \cdot i) > c(h, e).$$

I did not adopt $\mathfrak{C}'$ because it is too narrow. In particular, I mentioned the following two counter-examples in which (9) holds, but $\mathfrak{C}'$ does not hold :

(12) h is a law ' $(x)(Mx \supset M'x)$ ' (e.g. 'all swans are white') in a finite domain of N individuals ($N > 1$) ; e is tautological ; i is ' $Mb \cdot M'b$ ' (' b is a swan and b is 'white').

(13) e is ' $Pa_1 \cdot \sim Pa_2$ ', where ' P ' is a primitive predicate ; i is ' $Pa_3 \cdot Pa_4 \cdot \dots \cdot Pa_{12}$ ' ; h is ' Pa_{13} '.

I indicated (l. c., p. 467) that the defect of $\mathfrak{C}'$ is due to its being defined in such a way that it is in accord with all regular c -functions (in the sense of (11)). Now Bar-Hillel points out, correctly, that my definition of $\mathfrak{M}\mathfrak{C}$ is too narrow in just the same respect as $\mathfrak{C}'$. In the two examples (12) and (13), the relation (9) seems intuitively to hold. Therefore we should require of an adequate explicatum $\mathfrak{M}\mathfrak{C}$ that

$$(14) \mathfrak{M}\mathfrak{C}(h, e \cdot i, h, e).$$

However, on the basis of my definition, this does not hold. There are regular c -functions for which neither (10) nor the weaker condition

$$(15) c(h, e \cdot i) \geq c(h, e)$$

holds in the cases (12) and (13). It is due to this fact that neither $\mathfrak{C}'$ nor $\mathfrak{M}\mathfrak{C}$ covers the two examples, because $\mathfrak{C}'$ is in accord with all regular c -functions in the sense of (11), and likewise $\mathfrak{M}\mathfrak{C}$ in the sense of (7).

Thus the problem of finding an adequate explicatum in non-quantitative terms for the comparative concept of confirmation is at

¹ L. c., p. 465

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present unsolved. I shall not try here to construct an explicit definition of $\mathfrak{MC}$ which would constitute an adequate explication; but I shall indicate the direction in which a solution might be found.

2 *Requirements for c-Functions*

Let us leave aside for a moment the comparative concept and discuss c -functions. There are many relations among values of c which, although intuitively valid, do not hold for all regular c -functions, e.g. the inequality (10) in cases like (12) and (13). In order to make such relations provable, additional assumptions concerning c must be made. I shall list here six requirements R1-R6, which seem to me indispensable for adequacy. That is to say that any c -function which does not fulfil all of these requirements seems to me unacceptable as an explicatum for degree of confirmation, or, in other words, as a valid basis for inductive reasoning. On the other hand, these requirements do not constitute a sufficient condition of adequacy. There is an infinite number of c -functions which fulfil all of these requirements but nevertheless would generally be regarded as inadequate.

Every c -function is assumed to be based on an m -function (in the sense that $c(h, e)$ is defined as $m(e \cdot h)/m(e)$). The requirements may therefore be formulated in terms of m -functions. It is here assumed that all primitive predicates designate properties, not relations, and that the number of individuals is finite.

- R1. m is *regular* (i.e. the m -values for the state-descriptions are positive and their sum is 1, see [Prob.] § 55A).
- R2. m is *symmetrical with respect to individual constants*. If i' is formed from the state-description i by exchanging two individual constants (i.e. by replacing each occurrence of the one constant in i by the other constant and vice versa), then i and i' have equal m -values.

[If this condition is fulfilled, then any two isomorphic state-descriptions have equal m -values.] The requirement R2 means in effect that all individuals are treated on a par.

- R3. For any sentence i without variables, $m(i)$ has the same value in all language-systems in which i occurs, irrespective of the number of individuals in the system. (In technical terms, the functions $_N m$ for $N = 1, 2$, etc. form a fitting sequence, see [Prob.] § 57C.)
- R4. If one state-description is formed from another by exchanging two primitive predicates (i.e. by replacing each occurrence of the

one predicate by the other and vice versa), then both state-descriptions have the same m -value.

- This means that all primitive properties are treated on a par.
- R5. If one state-description is formed from another by exchanging one primitive predicate and its negation (i.e. by adding a sign of negation to each component containing this predicate and then deleting double negation signs), then both state-descriptions have the same m -value.

This means that every primitive property is treated on a par with its contradictory.

- R6. *Requirement of instantial relevance.* Let ' M ' be a factual, molecular predicate. Let e be a non-L-false sentence without variables. Let i and h be full sentences of ' M ' with two distinct individual constants which do not occur in e . Let k_0 be $e \cdot \sim i \cdot \sim h$, k_1 be $e \cdot i \cdot \sim h$, and k_2 be $e \cdot i \cdot h$. Then $m(k_0)/m(k_1) > m(k_1)/m(k_2)$.

This requirement will hardly appear immediately plausible. However, it leads to the following theorem T1a (from which its name is derived); and this theorem seems plausible. (T1a is, moreover, equivalent to R6 on the basis of R1 and R2; this can easily be shown by reversing the subsequent proof.)

- T1. *Theorem of instantial relevance.* Let c be based on m , and m fulfil R1, R2, and R6. Let ' M ', e , i , and h be as in R6.

- $c(h, e \cdot i) > c(h, e)$; in other words, i is positively relevant to h on e .
- $c(h, e \cdot \sim i) < c(h, e)$; $\sim i$ is negatively relevant to h on e .
- $c(h, e \cdot i) > c(h, e \cdot \sim i)$.

Proof. a. Let k_0 , k_1 , and k_2 be as in R6; let k_1' be $e \cdot \sim i \cdot h$. Then, by R2, $m(k_1) = m(k_1')$. We put $m_0 = m(k_0)$, $m_1 = m(k_1) = m(k_1')$, and $m_2 = m(k_2)$. These three values are positive (R1). According to R6,

$$m_0/m_1 > m_1/m_2.$$

Hence

$$\begin{aligned} m_0 m_2 &> m_1^2; \quad m_0 m_2 + 2m_1 m_2 + m_2^2 > m_1^2 + 2m_1 m_2 + m_2^2; \\ m_2(m_0 + 2m_1 + m_2) &> (m_1 + m_2)^2; \\ m_2/(m_1 + m_2) &> (m_1 + m_2)/(m_0 + 2m_1 + m_2). \end{aligned}$$

Now

$$c(h, e \cdot i) = m(e \cdot i \cdot h)/m(e \cdot i) = m_2/(m_1 + m_2).$$

And

$$c(h, e) = m(e \cdot h)/m(e) = (m_1 + m_2)/(m_0 + 2m_1 + m_2)$$

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(see [Prob.] T65-1). Hence the assertion (a).—*b*. From (a), according to [Prob.] T65-6e.—*c*. From (a) and (b).

T1a says in effect that one instance of a property is positively relevant to the prediction of another instance of the same property. This seems a basic feature of all inductive reasoning concerning the prediction of a future event. We can easily derive from T1a the result that a conjunction of one or more instances of a property is positively relevant to a conjunction of one or more other instances. Thus, the second counter-example (13) is covered by any *c* which fulfils our requirements.

The following is a corollary of T1c : If e_M describes a sample of s individuals, of which s_M are *M* and the remaining $s - s_M$ are non-*M*, then $c(h, e_M)$ increases with increasing s_M . This means that the inductive probability of a future instance of *M* is the higher, the greater the relative frequency of *M* among past observations. T1c is equivalent to T1a (on the basis of R1 and R2) ; hence it could likewise be taken as a requirement instead of R6.

3 *Axioms for the Comparative Concept*

Various axiom systems for the comparative concept of confirmation or inductive probability have been constructed. Those by Keynes¹ and Jeffreys² form the basic part of a quantitative system. The most elaborate and strongest comparative system so far constructed is that by B. O. Koopman.³ But even the latter system covers only those cases in which the quantitative condition (3) is fulfilled for all regular *c*-functions (see [Prob.] p. 342 and § 83B) ; thus, e.g. it does not cover the two examples (12) and (13) and the cases referred to in T1. (This is easily seen from the fact that Koopman's axioms refer, not to the internal structure of the sentences involved, but only to deductive relations among them ; for T1, on the other hand, it is essential that the sentences *i* and *h*, which are deductively independent, are full sentences of the same predicate.) For Koopman this restriction is not accidental ; it is in line with his basic conception. According to this conception, 'it is the unique function of

¹ John Maynard Keynes, *A Treatise on Probability*, London and New York, 1921 (see my comments, [Prob.] pp. 338f.)

² Harold Jeffreys, *Theory of Probability*, Oxford, 1939 (see [Prob.] pp. 340-342.)

³ B. O. Koopman, 'The axioms and algebra of intuitive probability', *Annals of Math.*, Ser. 2, 1940, 41, 269-292 ; 'The bases of probability', *Bull. Amer. Math. Soc.*, 1940, 46, 763-774

such a theory [a rational mathematical theory of probability] to develop the rules for the derivation of comparisons in probability from other comparisons in probability previously given'.¹ In distinction to this conception, I would regard it as the task of a comparative axiom system to supply also direct comparative statements in addition to merely conditional statements.

If we wish to construct an axiom system which is not limited in this way, then we have to add axioms which are not in accord with all regular c -functions. I would suggest to add for this purpose to an axiom system of the weak kind, e.g. the one by Koopman, the subsequent axioms A1-A4. They correspond to our earlier requirements R2, R4, R5, and T1a (as an equivalent to R6) for c -functions. (We omit R3, because the present axiom system is meant to apply to one language-system only.) We use ' $\mathfrak{MC}$ ' as the primitive term. For the sake of convenience we make use of the definitions (4) and (16) :

$$(16) \mathfrak{Cq}(h, e, h', e') =_{\text{Df}} \mathfrak{MC}(h, e, h', e') \text{ and } \mathfrak{MC}(h', e', h, e).$$

In the intended interpretation, the comparative concept hereby defined corresponds to the quantitative condition :

$$(17) c(h, e) = c(h', e').$$

- A1. *Symmetry with respect to individual constants.* Let e be not L-false. Let h' and e' be formed from h and e , respectively, by exchanging two individual constants. Then $\mathfrak{Cq}(h, e, h', e')$.
- A2. *Symmetry with respect to primitive predicates.* Let e be not L-false. Let h' and e' be formed from h and e , respectively, by exchanging two primitive predicates. Then $\mathfrak{Cq}(h, e, h', e')$.
- A3. *Symmetry with respect to a primitive predicate and its negation.* Let e be not L-false. Let h' and e' be formed from h and e , respectively, by exchanging one primitive predicate and its negation. Then $\mathfrak{Cq}(h, e, h', e')$.
- A4. *Instantial relevance.* Let ' M ' be a factual, molecular predicate. Let e be a non-L-false sentence without variables. Let i and h be full sentences of ' M ' with two distinct individual constants which do not occur in e . Then $\mathfrak{Cq}(h, e \cdot i, h, e)$.

The following theorem is based on A4 together with customary comparative axioms.

¹ L. c. (the first paper), p. 271

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T2. Let 'M', e , i , and h be as in A4.

- a. Lemma. $\mathfrak{MC}(h, e \cdot i, h, e)$. (From A4 and definition (4).)
- b. Lemma. Not $\mathfrak{MC}(h, e, h, e \cdot i)$. (From A4 and (4).)
- c. Lemma. $\mathfrak{MC}(\sim h, e \cdot \sim i, \sim h, e)$. (From (a), with ' $\sim M$ ' for ' M '.)
- d. Lemma. Not $\mathfrak{MC}(\sim h, e, \sim h, e \cdot \sim i)$. (From (b), likewise.)
- e. $\mathfrak{MC}(h, e, h, e \cdot \sim i)$. (From (c) and the axiom of negated hypotheses (Koopman's axiom A, [Prob.] T83-33).)
- f. Not $\mathfrak{MC}(h, e \cdot \sim i, h, e)$. (From (d), likewise.)
- g. $\mathfrak{MC}(h, e \cdot i, h, e \cdot \sim i)$. (From (a), (e), and the axiom of transitivity (Koopman's axiom T, [Prob.] T83-22).)
- h. Not $\mathfrak{MC}(h, e \cdot \sim i, h, e \cdot i)$. [Indirect proof. Assume that $\mathfrak{MC}(h, e \cdot \sim i, h, e \cdot i)$. Then with (a) and the axiom of transitivity : $\mathfrak{MC}(h, e \cdot \sim i, h, e)$, which is impossible (f).]

T2(e) and (f) say that $\text{Gr}(h, e, h, e \cdot \sim i)$, which corresponds to T1b: the negation of one instance of M is negatively relevant to another instance. The theorems (g) and (h) say that $\text{Gr}(h, e \cdot i, h, e \cdot \sim i)$, which corresponds to T1c: a future instance of M is the more probable, the greater the observed relative frequency in the past.

4 The Problem of an Explicit Definition for the Comparative Concept

The definition of the comparative concept $\mathfrak{MC}$ given in my book was too narrow, as we have seen, because it was constructed in such a way that the defined relation was in accord with all regular c -functions. The defined concept was 'proposed merely as a tentative explicatum' (p. 453). And I indicated what ought to be done in case the defined concept were found to be too narrow: 'It would be of interest to investigate the possibility . . . of an explicatum which is wider than $\mathfrak{MC}$ [as then defined] because based on a narrower class of c -functions' (p. 467). Thus the task would be to choose a suitable narrower class C of c -functions and then try to find a definition of $\mathfrak{MC}$ which, although it makes no reference to c -functions or other numerical functions, is in accord with all c -functions in C and, moreover, is the most extensive concept in accord with all c -functions in C . In other words, $\mathfrak{MC}$ would be required to fulfil the following condition:

- (18) For any sentences h, e, h', e' , $\mathfrak{MC}(h, e, h', e')$ if and only if, for every function c in C , $\bar{c}(h, e) \geq c(h', e')$.

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It seems to me that a suitable choice for *C* might be the class of those *c*-functions which fulfil the requirements R1-R6. In connection with the above quotation, I suggested for the explication of both the classificatory and the comparative concepts to add to the requirement of regularity (R1) at least those of symmetry with respect to individual constants (R2) and of symmetry with respect to basic matrices (R4 and R5). Now the discussions in this paper have shown that it is essential to add also the requirement of instantial relevance (R6), in order to make sure that the explicata cover the important class of cases of the predictive inference. To the passage quoted above I added: 'However, . . . it seems doubtful whether a simple definition in L-terms can be found'. The discussions in this article will make these doubts even stronger. At any rate, it seems clear, in view of the six requirements, that the definition cannot have a simple form. It will have to refer, in addition to deductive relations (L-concepts) between the sentences involved, also to the internal structure of the sentences, as the above requirements do. Whether it is at all possible to find a definition which is non-quantitative, i.e. does not contain numerical variables, must remain an open question at the present moment. I am in this respect not quite as pessimistic as Bar-Hillel. If we permit the use of variables for expressions and maybe for classes of expressions (though not for numbers), then I think the task may be possible although not easy. To find a solution is chiefly of interest to those who need a comparative concept because they believe that it is impossible to find an adequate explicatum for the quantitative concept of degree of confirmation.

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WHAT DO WE MEAN BY 'INBORN'?*

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THE persistence of confusions about this question, even among biologists, suggests that the source of the difficulty lies rather deep, and that a satisfactory answer will only be found by a rather indirect route. This I shall try to show is the case.

But first we must decide the question: What sort of things are people talking about when they say that these things are inborn? The usual answer is that they are talking about things called *characters*. If we are not satisfied with this answer, but persist in asking what sort of things characters are, then we receive very unsatisfactory answers. People use the word 'character', like the word 'property', as a matter of verbal habit without ever enquiring how it works. This suffices for the situations of everyday life, but that is no guarantee that it will suffice for the demands of science. It seems, therefore, that if we are to find a satisfactory solution of our problem it will be necessary to eliminate the word 'character' from our formulations as well as the word 'inborn'. This may seem drastic, but the fact that confusions still persist concerning these questions, even in biological literature, suggests that any remedy will be drastic. Neither are such difficulties peculiar to biology. At one time physics suffered from confusions regarding 'warmth' and 'temperature'. Such linguistic teething troubles can be overcome by patient analysis if we are prepared to acknowledge, and are willing to deal with, them.

Every normal English-speaking person who has reached a certain age will understand what is being asserted when he reads:

Tom has red hair,

sufficiently well to be able to know (if he is acquainted with Tom) whether to accept or to reject this statement. But the mass of red hair which springs from Tom's scalp is not a character but a concrete particular part of a concrete particular boy. When people say that red hair is inborn because it is 'handed down from parent to offspring' or 'transmitted by heredity' they cannot be referring to concrete particular objects. For Tom's mass of red hair could not

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have been handed down as a wig of red hair might have been handed down. Tom's red hair is not a character, it is the *redness* which is the character and is 'possessed' by the hair, which is again 'possessed' (but in a different sense) by Tom. Tom's hair is a part of him but its redness is not part of his hair. Redness is an elusive and abstract entity. To say that redness is handed down from parent to offspring is clearly metaphorical; for no one supposes that a parent does or could hand down an abstract entity like redness to a child in the sense in which he could hand down a concrete thing like a watch. However harmless such metaphors may be in some contexts, we cannot be content with them if our aim is to give a clear answer to the question which forms the title of this article.

One way out of this difficulty which may suggest itself to the reader is to have recourse to current theoretical names in biology and to speak about genes. But such a procedure would not solve our original problem; for this is not concerned with any particular scientific explanation but with a question which is close to the observational level. Long before the notion of gene was introduced the word 'inborn' was in use. Moreover, if we appeal to theoretical names we must still explain their connection with names appropriate to the observational level and this explanation will be assisted if the latter words are clear. It would be a misuse of theoretical notions to employ them as a cover for slovenly formulations on the observational level.¹ Our task is to make everything on the observational level as clear as we can, because this is both the starting point and the final testing ground for any theoretical construction.

When I speak of the observational plane I have in mind such statements as 'Tom has red hair', but it will not be possible on the present occasion to use statements of this kind exclusively. It will be necessary to speak about organisms before they are born or hatched, and statements about such things are theoretical in more senses than one; in particular they often depend upon observations with the microscope, and the interpretation of such observations involves the optical theory of the microscope. It will be necessary to speak about zygotes (or fertilised eggs) developing into adults or into something less mature than adults (some organisms die young). In order to have a more general term to cover all cases I shall call them *lives*.

¹ In any case an appeal to this theory would not solve the present problem because, as will be seen below, genes are just as much involved in the non-inborn as in the inborn. The distinction cannot be explained on such a basis.

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Thus a life (in the sense of the present article) is a time-extended organism from the moment of fertilisation to some moment later, up to and including the moment of death.

Instead of spending more time discussing existing formulations, I proceed now to the more profitable occupation of offering an alternative one which can be judged on its own merits and may constitute an improvement. The use of words is so much a matter of unnoticed habit that some discussion of existing formulations has been necessary, with the intention of creating dissatisfaction with them and an appetite for something different.

I should like to be able to formulate my explication of 'inborn' in plain English. But such an explication, if given in the first instance, would be exceedingly long-winded and difficult to follow. (This is only to be expected. Were a formulation in plain English easy it would have become familiar long ago and there would have been no need to write this article.) I shall therefore first formulate what I have to say in reinforced or supplemented English, namely English helped out with certain devices belonging to the theory of sets. Anyone who then wishes to expand it into plain English will have all the means necessary for doing so at his disposal.

The principal deviation from plain English to be adopted is the use of letters as dummy names (or variables as they are usually, if unfortunately, called). I shall use small letters like ' x ', ' y ', ' z ' as dummy names which can have the names of individuals substituted for them, and capital letters, ' X ', ' Y ', ' P ' as dummy names representing names of sets of individuals. I shall also adopt two abbreviations which are in current use and will help to make statements shorter and easier to read. These are ' ϵ ' to represent set-membership, and ' $\subset$ ' for set-inclusion. Thus instead of saying ' x is a member of the set X ', or ' x belongs to X ', I shall simply write ' $x \epsilon X$ '. And instead of 'every member of X is a member of Y ', or ' X is included in Y ', or ' X is a sub-set of Y ', I shall simply write: ' $X \subset Y$ '. Finally, I shall use the device of writing a bar over a set designation in order to denote the complement of the set. Thus ' $\bar{X}$ ' will denote the set of all things (in the universe of discourse) which do *not* belong to X .

I begin by introducing the phrase :

x develops in y into z

which becomes a statement as soon as the letter 'x' is replaced by a designation for some particular zygote, 'y' by a designation for some particular environment, and 'z' by a name for some particular life. It becomes a *true* statement if this particular life begins with the zygote whose name is substituted for 'x', and if it develops in the particular environment whose name is substituted for 'y'. As an abbreviation for the above expression I use :

$$\mathbf{dlz}(x, y, z).$$

With the help of this it is now easy to define a corresponding relation not between individuals but between *sets* of individuals. I write :

$\mathbf{Dlz}(X, Y, P)$, (which may be read : Every X which develops in a Y does so into a P) ; if and only if : for all x, y and z , if $\mathbf{dlz}(x, y, z)$ and $x \in X$ and $y \in Y$ then $z \in P$.

Next suppose we have a set X of zygotes, a set Y of environments and a set P of lives which satisfy the following conditions :

$\mathbf{Dlz}(X, Y, P)$ and $\mathbf{Dlz}(\bar{X}, Y, \bar{P})$ and $\mathbf{Dlz}(X, \bar{Y}, \bar{P})$ and $\mathbf{Dlz}(\bar{X}, \bar{Y}, \bar{P})$

so that *only from* a member of X and *only in* a member of Y do you get a P . Then I shall say that X is the *zygotic range* of P , and I shall call Y the *environmental range* of P . For brevity I shall write : $X = \mathbf{Z}(P)$ and $Y = \mathbf{E}(P)$.¹

Suppose further that ' $\mathbf{A}(P)$ ' denotes the set of all lives which have an age or time length equal to that of the adult members of P and that W is a set of environments which satisfies the following conditions :

$$\mathbf{Dlz}(\mathbf{Z}(P), W, \mathbf{A}(P)) \text{ and } \mathbf{Dlz}(\mathbf{Z}(P), \bar{W}, \bar{\mathbf{A}}(\bar{P}))$$

then we can call W the $\mathbf{A}$ -environmental range of P and write $W = \mathbf{EA}(P)$.

After these explanations and preparations we are now in a position to make a first attempt to formulate one of the crucial definitions which will help us to solve our problem. We shall say (provision-

¹ It must be understood and remembered that in practice all the sets of zygotes, of environments and of lives here mentioned will be *specified* in some way which does *not* involve ' $\mathbf{dlz}$ '. P for example might be specified by the statement : 'belongs to *Pisum sativum* and has green cotyledons'. In this case a species name, a part name and an adjective ('green') are used. But it does not seem to be at all necessary to speak of characters or of greenness.

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ally) that a set P of lives is *environmentally insensitive* if and only if we have :

$$\mathbf{EA}(P) \subset \mathbf{E}(P)$$

that is to say if *every* environment in which a member of the zygotic range of P develops into an adult of the same age as adult members of P is *also* an environment in which it develops into a P .

This definition has the disadvantage of being applicable only to adults. But we want our definition to be more general so that it will cover sets P which are such that it is possible for a life to be a P *before* reaching the adult time length. Our last step therefore consists in removing this restriction.

Suppose that P is a set of lives and we denote by ' $\mathbf{Ti}(P)$ ' the set of all lives which have a time length which is equal to or greater than that of the youngest member of P . If W is a set which satisfies the following conditions :

$$\mathbf{Dlz}(\mathbf{Z}(P), W, \mathbf{Ti}(P)) \text{ and } \mathbf{Dlz}(\mathbf{Z}(P), \overline{W}, \overline{\mathbf{Ti}(P)})$$

then W will be called the $\mathbf{Ti}$ -environmental range of P and we write $W = \mathbf{ETi}(P)$.

Our definitive definition can now be given as follows :

P is *environmentally insensitive* if and only if

$$\mathbf{ETi}(P) \subset \mathbf{E}(P)$$

which means that *every* environment in which a member of the zygotic range of P develops into a life having the appropriate time length is *also* one in which that zygote develops into a P .

On the other hand, we say that P is *environmentally sensitive* if and only if it is *not* the case that

$$\mathbf{ETi}(P) \subset \mathbf{E}(P),$$

in other words if there exist environments in which a member of the zygotic range of P attains the requisite time length but *without* developing into a member of P .

Now what I want to suggest is that there would be fewer misunderstandings in biological science, and fewer misleading statements in popular biology, if people ceased to speak of inborn and acquired characters and spoke instead of environmentally insensitive and environmentally sensitive sets of lives. (Note that being environmentally insensitive does *not* mean that a zygote of the required zygotic range will develop into a member of the set in question in boiling oil ; it means *only* what the definition says it means, not whatever the words may suggest).

I am encouraged to believe that some authorities will agree with me by the following passage from Darlington and Mather's *The Elements of Genetics*, London, 1949, p. 15. They say :

Leaves may be yellow on account of the conditions under which the plant is grown, that is on account of the *environment*. But we also know plants that have yellow leaves under any conditions. These must be yellow on account of their internal or inborn properties.

It is clear that these authors, when they use the phrase 'plants that have yellow leaves under any conditions' do not intend it to be taken quite literally without qualification. They clearly mean it to be qualified, although they do not state what the required qualification is. And it must be qualified because no seeds are known from which plants with yellow leaves will develop in strong alcohol or strong sulphuric acid or water at 100°C. We cannot therefore be said literally to know plants that have yellow leaves under any conditions ; we must say under *certain* conditions. Now the whole purpose of my string of definitions leading to the notion of environmentally insensitive sets was to make explicit the reservations that must be put upon the phrase 'any conditions'.

The foregoing exposition of environmentally insensitive and environmentally sensitive suggests that there should be an analogous pair of notions, namely, zygotically insensitive and zygotically sensitive. These can be defined in the following way. Let ' S ' denote the species in which the set P is included, and let ' $S \cap \mathbf{Ti}(P)$ ' denote those members of S which also belong to $\mathbf{Ti}(P)$. Then we can say : X is the $\mathbf{Ti}$ -zygotic range of P or $X = \mathbf{ZTi}(P)$ if and only if

$$\mathbf{Dlz}(X, \mathbf{E}(P), S \cap \mathbf{Ti}(P)) \text{ and } \mathbf{Dlz}(\bar{X}, \mathbf{E}(P), \overline{S \cap \mathbf{Ti}(P)}).$$

Finally we can say that P is *zygotically insensitive* if and only if :

$$\mathbf{ZTi}(P) \subset \mathbf{Z}(P).$$

This says that a set P of lives is zygotically insensitive provided every zygote which develops into a member of S and of $\mathbf{Ti}(P)$ in an environment belonging to the environmental range of P also develops into a P in that environment.

Correspondingly P is *zygotically sensitive* if and only if there are some zygotes which develop into members of S and of $\mathbf{Ti}(P)$ in environments belonging to $\mathbf{E}(P)$ but without developing into members of P .

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The set of all English-speaking people is perhaps an example of a zygotically insensitive set. This notion may be important in sociology, playing a part there which is analogous to the part played by the notion of environmentally insensitive in genetics.

It will be noticed that, in a sense, our definition of environmentally insensitive is a purely embryological one, not involving genetical considerations at all. It presupposes that it is possible to specify sets of zygotes, of environments and of lives in some way, but no question is begged regarding the *way* in which they are specified. Questions concerning 'handing on from one generation to another' are not explicitly or even implicitly involved in distinguishing between environmentally sensitive and environmentally insensitive. They are introduced only if it is required that the zygote sets must be *parentally* specified (that is, specified by reference to a classification of the parents who supplied the gametes by the union of which the zygotes concerned were formed). But this may be demanded of *any* zygote set, whatever sort of lives its members may develop into, whether environmentally sensitive or insensitive. This is a point worth reflecting upon. It may also be put in another way. It will be seen that ' $Z(P)$ ' cannot be omitted from the definition of $ETi(P)$ or of $EA(P)$ without making nonsense of the whole thing. This is obviously because, in order to get a life of sort P , *whether this is environmentally insensitive or not*, it is necessary to have a zygote within the zygotic range of P and an environment within the environmental range of P ; the only difference is that in the one case the environmental range is as wide as is compatible with development at all, and in the other case it is not so wide. This point is made clear and kept before the attention by the use of the three-termed relation $Dlz(X, Y, P)$.

The practical importance of misunderstandings of 'inborn' is great both in medicine and in criminology. This is because it may be identified with 'incurable' in the one case and with 'incurrigible' in the other. It is obvious that the word should be used with great caution in such contexts because its use always involves a hypothesis. We cannot in fact try *all* environments; we have not yet found *all* cures or tried *every* system of education. We may be powerless to prevent events taking a certain course, but this does not always mean that the situation is hopeless. The development of red hair may be unpreventable, given a zygote of the appropriate zygotic range. But the person whose hair it is can, if he wishes, use a razor and wear a

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wig. It might be a misfortune to interpret a scientific theory in such a way as to discourage experiments in education and in the treatment of illness.¹

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¹ The foregoing article is an extension of, and I hope an improvement upon, my treatment of the same topic in my *Biology and Language*, Cambridge 1952.

In the above discussion I have not referred to the method of avoiding the confusions mentioned by speaking of genetic and environmental *differences* instead of characters. This is certainly an improvement and no doubt suffices in many contexts. But it is far from being an ideal solution or clarification because it still leaves us with abstract entities. Chalk differs from cheese but is there a third entity called their difference *between* them? Is genetics really concerned with the classification of such things? Only when *A* and *B* belong to some number system in which subtraction has been defined can we say that the difference between *A* and *B* is a third entity of the same kind as *A* and *B*. The difference between chalk and cheese is neither chalk nor cheese nor a physical object of any kind. Neither is the difference between a red-eyed and a white-eyed *Drosophila* a *Drosophila* or an eye.

DUALISM AND LANGUAGE *

LAN FREED

MOST philosophers rightly or wrongly imagine that by the exercise of their craft they are, if only incidentally, promoting the habit of clear reasoning. Even those sages who have set out to disclose the true nature of Reality or the real nature of Truth have usually claimed for their systems the merit of being more consistent and logical, as well as more illuminating, than other people's. Not even Hegel recommended his *Weltanschauung* on the specific grounds of its confusingness, and although in our day we have had Existentialism there is as yet no self-styled school of Inconsistentism. Still, it is only in the comparatively recent history of Western philosophy that serious attempts have been made to dispose of mysteries and problems not by propounding more or less elegant and satisfying explanations but, as it were, by purely mental treatment, dissipating them altogether through the methods of logical analysis. Hobbes's demonstration that Aristotle, by hypostatizing *est*, had made 'being' into a 'thing' was a landmark in the development of this method, and since then the recognition that many of the conundrums which had teased and fascinated thinkers for thousands of years were purely or mainly verbal in origin has kept philosophy busy among the accumulations of its ancient stock, rearranging, sorting, and scrapping—and sometimes taking back again. The so-called 'so-called' problem of Free Will has been shown to have arisen chiefly from the error of talking about the will as though it corresponded to a bodily member, which must either be free to move about, or else not, as when tied to a post or set rigid in plaster of Paris. However, it must be admitted that not everybody is even now satisfied that the last word has been said on this subject. It was indeed only to be expected that new methods would give rise to new problems, and although these may be called problems of interpretation only, there is no denying that the ancient mysteries have a remarkable vitality, and a knack of tunnelling underground and reappearing in unexpected places.

One question which continues intermittently to vex the philosophic mind is that of the relation, if any, between Mind and Matter.

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Nobody, with the doubtful exception of those who managed to formulate their own distinctive solutions, could feel satisfied, at least until quite recently, either that the problem had been finally disposed of, or that it never existed at all. I want here to draw attention to some of the reasons for its peculiar intractability.

Mind-matter dualism, which some people have unjustly ascribed to Descartes, dates back, of course, much farther than philosophy : farther, probably, than any kind of systematic speculation about the nature of man and the universe. On the evidence of anthropologists—and perhaps also from what we have observed, or can remember directly, of the attitude of the child towards large important objects in his neighbourhood—it seems probable that the belief in indwelling spirits, or souls or minds, was not originally a hypothesis put forward to explain anything, but rather a spontaneous ‘feeling’ about all objects sufficiently distinctive to give one a sense of their individuality. Primitive man seems not to have drawn any sharp distinction between the animate and the inanimate as we have come to do, and for him everything not completely insignificant had its own spirit which, often, was capable of leaving the body which it inhabited and returning to it again. Thus the body and its ‘soul’ normally interacted, but the soul could on occasions act independently. In many parts of the world such views persist to this day, but elsewhere, as we know, the soul presently came to be regarded as belonging to living creatures only, and finally to man alone. It is a curious fact that whilst much speculation has been devoted to the problem of how men came to believe in souls, no systematic attempt has been made to trace the steps whereby they eventually learnt to limit and restrict this conception so drastically. This would surely be a most interesting line of research for the psychologist and anthropologist in collaboration. Meanwhile we can imagine the supplanting of animistic dualism by more sophisticated modes of thought as having proceeded along two main, roughly parallel, paths, which, however, continually intersected each other.

First, one may suppose that from the desire of man to control things, and the desire of some men to control other men, there presently grew the notion of laws : laws which required obedience of both things and men, and which embodied the demands of an overriding Soul or Spirit whose behests the lesser spirits had to obey. In this way a part of mankind would have learnt by degrees to disregard the subservient lesser spirits and finally to disbelieve in them

altogether, seeing the whole of nature as permeated by, or under the tutelage of, a supreme being who might be induced by appropriate actions on the part of men to alter the course of events in their favour. This must have represented a much more reassuring state of affairs than a clutter of small spirits, often in conflict among themselves and even if well-disposed, relatively impotent. Also the magician and the witch-doctor could now evolve into the priest or prophet, the man of such peculiar wisdom and virtue that he had been chosen by the great Spirit to interpret his will to the people. The emergence of such systems of belief need not have demanded any violent break with the past, since the great spirit was at first the governor of the still-extant lesser spirits.

Yet in evolving cultures the reorientation of thought proved momentous, for the Great Spirit ruled over, or was immanent in, Everything, and this directed men's minds outwards to speculations about nature and the cosmos.

Associated with the desire to control nature was the desire to understand, and once it became possible to regard events as happening not because of the unpredictable caprices of a multitude of spirits, but in accordance with some general plan conceived in a sort of supermind the way lay open, for those who cared to follow it, to a search for causes no longer *immediately* psychological: for the detailed working of the universal 'plan'. Along this second path the free-ranging speculative mind pursued its quest for a coherent picture of a universe which, after all, was as it was and worked as it worked whether or not under the control of a divine planner. The mechanistic view of the material world could be, and in due course was, carried to the extremest lengths—as by Descartes—without repudiating the First, or the Final Clause. Only, whilst few took it upon themselves to banish God from the universe, in the matter of interpreting physical processes He became gradually reduced to a cypher, until a time came when hardly anybody was shocked at the omission of His name from the textbooks.

It was quite another matter when the mechanistic searchlight was suddenly turned upon man himself. The uproar produced by Lamettrie's *L'Homme Machine*, which advanced the proposition that the soul is a product not of divine afflatus but of bodily growth, not only reflected the natural indignation of the churches at this challenge to their chief *raison d'être*, mediation between the human soul and its divine Author, but also the violent resentment of the common man,

who was led to believe, quite unjustly, that Lamettrie had said that his soul was 'nothing but dirt'.

Whilst the Materialists continued to be reviled for their degradation of everything most sacred in human life, scientific research was all the time serenely proceeding to reveal more and more about the physical springs of human action, watched with growing interest by the philosophers who, according to temperament, reacted with dismay or with enthusiasm to the accumulating evidence that human activity could be regarded as a process like any other. The bearing of all this upon the venerable problem of the mind-matter relationship was quickly perceived, and, under the influence of radical minds such as T. H. Huxley's in the nineteenth century and J. B. Watson, founder of the Behaviourist school in the twentieth, some people, including several eminent thinkers, became convinced that the solution lay in the total discarding of all psychological concepts such as consciousness, reasoning, and imagination. Only by such means could 'soul' be finally dislodged from its last remaining stronghold. For merely to embrace some version of the Lamettrian interpretation, or to play-down the concept of the individual and private 'self', was not to get rid of the central dichotomy and, so long as the language of psychology was still current, science itself could go no further than to ascribe every thought, emotion, and disposition to a physical cause. And up sprang the ancient problem: how *could* a physical cause produce a psychological event? Manifestly one had to discard the whole complex of psychological concepts in order once and for all to show that the mind-matter problem was the product of false reasoning or, it would perhaps be better to say, the wrong use of words. Once get rid of Mind, and the mind-matter dualism would be no more. It was as simple as that, or nearly so.

That there existed serious obstacles to the carrying out of this programme might have been more easily noticed had it not been for the confused and emotional reactions of the opposition who, in an age when rationalism and science went for the most part hand in hand, seemed unable to find any better arguments than appeals to intuition and quotations from Holy Writ, with the effect of admitting that no better arguments could in fact be found. This gave colour to the view that only prejudice and atavistic modes of thought obstructed recognition of the fact that mind was a fiction which, when science had discovered only a little more, could and should be dispensed with.

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Yet after all, as it transpired, to expose the irrationalities of one's opponents was not to resolve the dichotomy in one's own mind, and by the time the more fanatical champions of Behaviourism had carried their doctrine to such extremes that they were to be heard chanting in unison with the Christian Scientists, 'Pain is not Real', their former allies in the camps of the philosophers were already beginning to desert them. Bertrand Russell, for example, who at one time showed a decided bias in their favour, committed himself in his *Human Knowledge* to the view that psychology and physics are different sciences with different subject-matters, and also remarked that to deny that there is any such thing as introspection is to make oneself ridiculous. This was symptomatic of the backwash towards acceptance, even if qualified and reluctant, of the ancient dualism.

Meanwhile our linguistic habits had come under fire, and it had seemed for a time as though the dichotomy might be somehow resolved by certain methods of logical analysis, but these hopes proved illusory. Logic, in so far as it must remain divorced, to quote a phrase of Carnap's, from 'the agreement of conceptions and the like', could not be applied to the solution of what was essentially a conceptual problem, and the various attempts at compromise collapsed in a welter of inconsistencies. Numerous monistic theories were all the time being tried, which sought to dissolve the distinction between subject and object, or observer and observed, but these proved satisfying to their authors alone.

And then, with rather the effect of a knight in shining armour riding to the rescue of a beleaguered garrison, came Professor Ryle with his scintillating *Concept of Mind*. The warmth of enthusiasm with which this truly dazzling book was received by both professional philosophers and laymen was a measure of the discouragement which had fallen upon all who had tried to come to grips with the problem, but it was also a tribute to the refreshing effect the book conveyed of ingenuity combined with simplicity of presentation, of masterly powers of analysis with robust common sense. It seemed to cut a clear path through the thickets of muddled thinking in which the subject had heretofore been entangled.

The trouble, it will be remembered, was, according to Professor Ryle, mainly if not entirely due to a 'Category Mistake': a great comprehensive howler which has fathered a whole brood of subsidiary howlers for our confounding. The mistake consisted of our practice of treating the mind as if it were a kind of non-material

entity, a man being composed of a mind contained in a body, a 'ghost in a machine'. This alone gave rise to such ridiculous pseudo-problems as that of whether some particular event had a mental or a physical cause.

We may recall some of the parables with which Professor Ryle illustrates the Category Mistake: A visitor to a University, who has been shown all the buildings and offices which go to compose it, then asks to see 'the University itself'. A child who has watched the march-past of a division composed of battalions, batteries, squadrons, etc., asks when 'the division' is going to appear. A foreigner at a cricket-match having had the functions of the various players explained to him, then wants to know which player has to contribute the necessary 'team-spirit'. These examples bring out the point that a person should not be regarded as consisting of two separate elements, body and mind, but as a single entity distinguished from other types of object by the fact that its actions are thoughtful, or thought-out actions.

This attitude seems, on reflection, to be quite fairly representative of the commonsense, non-philosophic point of view from which we do, more often than not, regard one another. Yet it must be admitted that although in our intercourse with our friends, or even when we encounter strangers, we do not as a rule make the mistake of wondering where their self resides, or enquire which part of them constitutes their personality, we are inclined in many sorts of circumstance to *concern* ourselves predominantly either with the physical or with the mental 'side' of a person, and more or less ignore the other 'side'. For example, to the question, 'What is your fiancé like?' the reply might well take the form of a list of physical attributes, as 'He has blue eyes and a red moustache'; but should the enquirer persist, 'Yes, but what kind of *person* is he?' the answer will probably be on the lines of, 'He has a wonderful sense of humour and he loves cats,' that is, an account of the person in psychological terms. A reply which conformed with Professor Ryle's recommendations would presumably be: 'He is always laughing and joking, and when he sees a cat he strokes it and says, "nice pussy",' because this involves no bifurcation of the man into separate mental and physical elements, but by saying how he *acts* treats him as one thing: a certain sort of man.

The difficulty is, however, that we do not, perhaps because we cannot, invariably think or talk of each other and of ourselves in this

way. (I think it is evident that the above alternative to the statement about the sense of humour and the love of cats will not do at all.) There is an almost infinite series of possible gradations in the aspects of a man from the purely psychological to the purely physical. If one looks straight at the tail-feather of a cock it appears to be black. If one begins to tilt it slightly in one direction it takes on a shimmer of colour until at a certain stage it is a pure brilliant green. Turn it back in the other direction through black and by degrees it becomes peacock blue. Even so one may look at a man according to Professor Ryle's prescription and he will be a perfect blend, neither preponderantly a mental nor a physical phenomenon, but if we are interested in his prowess as a boxer then we tilt him, as it were, albeit only slightly, to his 'physical' side. If we have him laid open on an operating-table we tilt him to the furthest physical extreme; he is like the feather in its all-green aspect. Moving in the opposite direction: if the proficiency of a secretary is under consideration, although interest is partly focused on her dexterity as a typist the question of her intelligence and her temperament is paramount. And if we are seeking the advice of an eminent philosopher on a problem that is troubling us then for the time being it is in his aspect as a thinker alone that we see him—all-blue, as it were. I think this metaphor of the feather more nearly represents our usual way of regarding each other than does the 'ghost in the machine' conception, but of course it does not point to any method for dispensing with our normal ideas of body and mind as 'constituents' of the whole person. Professor Ryle himself goes so far as to say at one point that it is 'perfectly legitimate to say in one logical tone of voice that there exist minds, and to say in another logical tone of voice that there exist bodies', but elsewhere it is manifest that he is still out to dissolve the distinction altogether, and we find him slapping-down the demon of dualism wherever its head bobs up through the interstices of his argument.

Thus in order to confute the view that reasoning is a particular distinctive activity belonging to the class 'mental', he writes: 'An argument is used, or a conclusion drawn, when a person says or writes "because this, therefore that", or "this involves that", *provided that he says or writes it knowing that he is licensed to do so*'. The phrase I have italicised, if it means anything, can only mean 'Provided he knows (or believes?) the argument or conclusion to be valid'. But to say this would be to admit the concept of a process of reasoning

prior to the use of the argument or the pointing of the conclusion, and it is just this concept that Professor Ryle is at pains to exclude. He goes on : ' This saying or writing in this frame of mind is, of course, a mental, indeed an intellectual act. . . . But this is not to say that it is a " mental act " in the sense that it is performed behind the scenes.' Since saying and writing are mentioned precisely as *examples* of activities which are not performed behind the scenes this does not take us very far. However, the main difficulty would seem to be that an act of saying or writing is not a mental act at all but a physical one. ' It may ', the argument continues, ' be done in silent soliloquy, but it may just as well be done aloud, or in ink '. . . . A ' mental act ' performed *in ink*? This sharply draws attention to the fact that category mistakes can be of more than one kind.

Professor Ryle finally drives his point home as follows : ' Indeed we expect to find a thinker's most subtle and most careful arguments . . . in what he submits for the criticism of his colleagues. We know what to suspect if a thinker boasts that he has a good argument which he will not or cannot publish.'

We may think that this particular argument might well have been left unpublished, but it does call attention to the sort of dilemmas which beset the philosopher who attempts the impossible task of analysing-away concepts which are a part of the very texture of his own thought. There is a frank admission in the preface to Professor Ryle's book that he is aware of being himself the victim of the assumptions against which he exhibits, as he says, ' such heat '. ' Primarily I am trying to get some disorders out of my own system.' Yet it is very evident that the physician has proved unable to cure himself, and perhaps this is the best possible proof that the complaint is very difficult indeed to cure. If so, the question still remains : What is the origin of the complaint? Or perhaps it would be better to say, why should it ever have occurred to us to complain of an attitude which our forefathers seem not to have troubled about at all?

Certainly a good measure of blame must be accorded the philosophers, who arrested, and proceeded to tamper with, a part of human thinking which spun along happily enough so long as nobody stopped and examined it. But of course, the process of abstraction and difficulty-making was far gone before Descartes ever turned his attention to the problem and produced his highly ingenious and unsatisfying solutions. If we have to find an arch-culprit it would be better to take Aristotle, who was the first to draw a sharp distinction between

the mechanical and instinctive side of the living organism, which he called the irrational soul, and which was related to the body as form is to matter, and the rational soul, or mind, which is 'an independent substance implanted within the soul and incapable of being destroyed'. It is true that in many of his references to the soul Aristotle seems to regard it very much as Professor Ryle regards, or tries to regard the mind, as when he writes: 'to ask whether soul and body are one is as meaningless as to ask whether the wax and the shape given it by the stamp are one'. But elsewhere he carefully distinguishes between the 'irrational soul', which belongs to all living entities, even vegetables, and the immortal 'mind'. He was thus the first great exponent of the doctrine of mind as a unique and distinct 'something' not subject to the laws governing matter, long before Descartes came upon the scene.

Yet Aristotle sometimes contradicts himself about the various 'parts' of the soul; he is undecided about where to draw his lines of demarcation between body-soul and soul-mind, and is evidently not quite satisfied with any of his solutions (though, to do him justice, he never falls back upon the device, much favoured by some of the early exponents of Behaviourism, of carefully avoiding the use of certain expressions which might compromise one's thesis, whilst making it necessary for the reader to 'think' the concepts which they ordinarily symbolise if he is to follow the argument at all).

Aristotle, for all his philosophy, had to think within the ultimate boundaries which enclose the thoughts of ordinary men, and neither he nor any other professional thinker can be accused of inventing the habit of introspection which, once contracted, formed the soil from which this and so many other problems were to spring. The concept of mind had its genesis in the thoughts of people surely incapable of regarding it as either creating, or solving, philosophical problems. Mind had its local habitations everywhere, no doubt long before it had its name, and to this day Descartes has remained unique in that he dared to exterminate the species throughout his territory, save where he allowed it to survive in its one preserve, its National Park, the human being. Now we are told that to be consistent we must get rid of it altogether. We must force ourselves to reject the notion that thought is *sui generis*.

'To what end?' we ask. And they answer: 'Why, to the end of avoiding mistakes in reasoning. For the clarification of our concepts—oh, you know what I mean!'

We do know what is meant. We have all suffered from the intellectual qualms due to entertaining the idea of both physical and mental processes, with all the seemingly insoluble difficulties of interpretation which this mode of thought involves. But once having noticed that we can consider these problems we should realise that, in one sense, we are committed. For when we come to think about it, we cannot but think that we think, and thinking is a mental, not a physical, activity. *Sum cogitans*.

This is not to say that thinking must be regarded as the product of a non-substantial 'thing', the mind; any more than willing is the product of a non-substantial 'thing', the will. As Otto Neurath remarked, when a watch stops we do not ask, 'Where has the Go gone?' 'Thinking' and its equivalents in other languages, is a term used by human beings for something that they find themselves doing, and that they cannot help, and that they cannot help noticing that they do, and perhaps it is only because each is aware that he carries on this process separately, privately, and even secretly that the term 'mind' was ever needed at all. No doubt it is just this privacy of thinking which produced the naive pictorial conception of a kind of cupboard in which thoughts are kept. 'Tell me what is in your mind', we say, hoping to be allowed a glimpse of the inside. But we know well that he is free to refuse, and hold it shut. That we may find means, from verbal persuasion to drugs and even to the use of physical violence for compelling him to open it, only lends substance to the metaphor. Yet after all, it should not be impossible to give up the habit of talking about minds. Some confusion might be avoided, though perhaps also created, if we were to do so. But who can believe that this would go any way at all towards overcoming our dualistic habit of thought? Is it reasonable to suppose that we can ever overcome it?

Along the lines of Solipsism a form of pure Idealism can, it would seem, be temporarily upheld, in private. Descartes found himself able to doubt even his own physical existence, and noticing that it was he who did the doubting he found himself obliged to believe that the doubt, and with it the doubter, existed. We may think of Descartes as having been at this stage in a genuinely Idealist mood—as thinking of nothing but himself as an incorporeal thinker thinking about himself thinking. This seems a possible state of mind, although difficult to keep up for long. Can it be more than a state of mind? Can it be argued for or against? I do not think it can; and I believe

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that the impossibility of upholding or opposing it was felt by Descartes himself (in the course of recording this early stage of his reflections, before ever he came to set down his arguments about perfection, God, and so forth). He showed this by the way in which he interpreted mind as a counterpart of matter or body. But those who charge him with making of mind a second body are apt to miss the point that having once set out to *communicate* his realisation of his thinking 'self' as an object of thought, he could hardly have helped himself. For communication must be based on aspects of the commonly-known, and just because of the essential privacy of self-awareness as an experience, any language in which we communicate about it will have primarily to employ expressions which are used to refer to the world of our common experiences of material things. We are compelled in speaking of our private experience to use metaphors, either veiled or overt, which are derived from our communications about the public material world, the meeting-ground of separate thought-systems (minds). On the intersubjective plane, I suggest, we are obliged to keep up a dualistic equilibrium, as it were, whose breakdown would amount to insanity, and which cannot be upset so long as normal communication takes place. This equilibrium means that communication presupposes a material world and equally a mental world. The individual, no doubt, is capable of imagining himself as affecting, by means of speech, a community of Robots reacting mechanically, though perhaps unpredictably, to his verbal pressures. He can be a Materialist in his attitude towards everything except his own experiencing 'self'. But if he ever admits, directly or by implication, that he holds intercourse with other people he cannot without inconsistency declare himself a Materialist. 'Social' Materialism is just as impossible as 'social' Idealism.

I think some evidence for the omnipresence of the Dualistic attitude may be seen in the way in which all our personal communications about things appear to carry an implicit 'I think so', or 'I believe so' with them. I do not want to suggest that, if I say to somebody 'it is snowing', this assertion is logically equivalent to 'it is snowing and I think it is snowing'. This theory, although it would establish the point I want to make, is probably quite untenable. Nevertheless it cannot be denied that if I say to somebody, 'It is snowing, *but I don't think so*', he will feel either that I am contradicting myself, or that I want to unsay what I have just said. For in ordinary communication an assertion about the material world is

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understood not only as such, but also as an indication, or an expression, of the thoughts or beliefs or states of mind of the speaker. (This does not hold, of course, for assertions produced by instruments such as barometers or talking clocks.) In this sense we may say that every ordinary communication-situation is, essentially, dualistic, involving a material world about which we speak *and* a speaker who knows or believes or thinks.

The view I have tried to sketch is, in essence, this : Thought has both a subject and an object. It is by . . . and it is about . . . That which it is by must be a thinker. That which it is about may be at moments the thinker's immediately intuited 'self'. This seems to allow the possibility of a pure Idealist attitude being privately entertained ; but the act of communication, no matter by what means (for example telepathy), appears to require the understanding-context of a *prima facie* Dualism, simply because the world about which we primarily communicate is the world *we know*, the material world which is the object of *our thought*.

I fully sympathise with the feeling that some form of Monism would be far more restful than the Dualism from which so many thinkers struggle in vain to escape. But after all, few of us believe that this is the best of all possible worlds : is there any more reason for thinking that it must be the intellectually most comfortable one ?

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THE MESCALINE PHENOMENA

J. R. SMYTHIES

HALLUCINATIONS occur in a great variety of medical conditions such as epilepsy and cerebral tumour, toxic deliria and various psychiatric conditions, and in the syndromes produced by the action of certain drugs upon the brain. In most cases the sanity and observational integrity of the victim may be called into doubt, and the phenomena may be dismissed as 'mere' fancies. The proper study of hallucination depends upon the choice of a non-toxic agent which will produce clear, vivid, and unmistakable hallucinations in a normal subject, and will at the same time leave his observational integrity intact and his critical judgment unclouded. He should in fact feel and appear perfectly normal except that his sense-fields should contain sensa which have no counterpart in the physical world. Although there are a number of well-known agents (for example hashish and opium) which will produce visual hallucinations, these have the obvious disadvantages of being dangerous habit-forming drugs and of clouding reason to some extent. There is only one agent which fulfils the conditions given above: the alkaloid, mescaline. The effects produced by mescaline are not widely known outside a small circle of psychiatrists and psychologists who have investigated its actions. Certainly, as far as I know, no one has commented on their importance for philosophy. It is my aim in this paper to give a brief account of its actions and to indicate their relevance to philosophy. My main purpose is to present the facts.

Mescaline, a vegetable alkaloid, is found in nature in the juices of a small Mexican desert cactus, *Anhalonium Lewinii*. The Mexican desert Indians prepare a brew, *peyotl*, from the plant and use this for their religious ceremonies. Scattered references to this practice may be found in the writings of the Spanish Jesuit priests of the seventeenth century, and the drug was known to the Aztecs as *teonatacatl*. It was only discovered for Western science in 1886 by the great pharmacologist, Lewin, after whom the cactus is named. His reports aroused a certain amount of interest at the time, and a number of investigators carried out research into its action. Weir Mitchell and Havelock

Ellis were among the most notable of these.¹ A second wave of interest in the drug was aroused by the publication in 1927 of Beringer's and Rouhier's monographs.² Since then mescaline has been used chiefly by psychiatrists who have taken it to undergo themselves something of the experiences of their schizophrenic patients.³ Rouhier, indeed, hoped that the drug would be of use in psychoanalysis, and Havelock Ellis expressed the opinion that a drug with such marvellous effects would soon become popular. But neither of these hopes have been realised, and this extraordinary substance remains in almost complete obscurity.

Mescaline has quite a simple chemical formula and its mode of action is well known. It interferes with the action of one particular enzyme in the brain which consequently becomes unable to use glucose properly. When taken by a normal subject it produces marked changes in perception, sensation, feeling, in some cases thinking, and in the relation between the ego and its environment, some of which changes are so remarkable as to defy description. It produces varied effects in different subjects, and the following account represents what may be expected to happen if a number of subjects are chosen. Each will experience in varying degree some different combination of the totality of possible effects. None will experience them all. A few will experience only the minimal reaction. There appears to be a correlation between the quality of the experiences and the personality of the subject. The unexpected features of the mescaline phenomena are, to make my point briefly, that the hallucinations or visions it produces are of surpassing beauty and possess the utmost poetical integrity.

Some two hours after taking mescaline the subject will begin to see, if he keeps his eyes closed, vague patches of colour floating about in his visual field. These soon develop into more complex *sensa*: mosaics, networks, flowing arabesques, interlaced spirals, wonderful tapestries, and patterns and designs of all sorts, all swiftly coming and going and all in the most beautiful colours and exquisite design. Then formed objects appear, great butterflies gently moving their wings,

¹ S. Weir Mitchell, 'Mescaline', *British Medical Journal*, 1896, 2, 1625 & Havelock Ellis, 'Mescal. A new artificial paradise', *Ann. Rep. Smithsonian Institute*, 1897, 1, 537

² A. Rouhier, *Le Peyotl*, Paris, 1927 & K. Beringer, *Mescalindrausch*, Berlin, 1927

³ H. Osmond and J. Smythies, 'Schizophrenia. A New Approach', *J. Ment. Sci.*, April 1952, 98, 309

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fields of glittering jewels, silver birds flying through silver forests, golden fountains and golden rain, masks, statues, fabulous animals, soaring architecture, gardens, cities, and finally human figures and fully formed scenes where coherent histories are enacted. If the subject opens his eyes the colours of objects become much more intense, deep, rich, and glowing, and they change their shape in curious and pleasing ways. One of my subjects, a highly intelligent and level-headed doctor, spent a quarter of an hour gazing at a plain glass full of water and trying to describe to me the perfection of its diamond brilliance. I myself was astounded by the heightened sparkle and glow of a wonderful mellow inner light that some wine glasses in a cabinet developed. One curious feature of this change is that it appears to be somewhat selective. Some objects become more beautiful than others. These are most likely to be the sort of simple object that a painter would choose to paint as a still life. An artistically worthless picture in a cheap magazine remains as ugly as ever, even though its colours may become richer.

These effects may not be explained by supposing that the subject is mentally deranged or 'drunk', as in most cases it is clear that he is perfectly normal in these respects. There is commonly no interference with the powers of careful observation and objective reporting. These inspected events are quite simply extremely beautiful. Everyone who has taken mescaline will make it plain that it is necessary to experience these phenomena oneself in order fully to understand them, and to realise that all the superlatives that may be used in an attempt to describe them are miserably inadequate. Having given that warning, let me present the following extracts from the accounts of the various investigators.

1. From Prentiss and Morgan :¹

Stretched out upon the bed with the eyes closed an everchanging panorama of infinite beauty and grandeur, of infinite variety of form and colour, hurried before me.

2. From Weir Mitchell :²

The display which, for an enchanted two hours followed, was such as I find it quite hopeless to describe in language which shall convey to others the beauty and splendour of what I saw. . . .

¹ D. W. Prentiss and F. P. Morgan, 'Anhalonium Lewinii', *Therap. Gazette*, 9, 577-585

² S. Weir Mitchell, *op. cit.*

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A white spear of stone grew up to a great height and became a tall, richly finished Gothic tower of very elaborate and definite design, with many rather worn statues standing in the doorways or on stone brackets. As I gazed every projecting angle, cornice, and even the face of the stones at their joinings were by degrees covered or hung with clusters of what seemed to be huge precious stones, but uncut, some being like masses of transparent fruit. . . . All seemed to possess an inner light, and to give the faintest idea of the perfectly satisfying intensity and purity of these gorgeous coloured fruits is quite beyond my powers. All the colours that I have ever beheld are dull in comparison to these. As I looked, and it lasted long, the tower became a fine mouse hue, and everywhere the vast pendant masses of emerald green, ruby reds, and orange began to drip a slow rain of colours. After an endless display of less beautiful marvels I saw that which impressed me most. An edge of a huge cliff seemed to project over a gulf of unseen depth. My viewless enchanter set on the brink a huge bird claw of stone. Above from the stem or leg hung a fragment of some stuff. This began to unroll and float out to a distance, which seemed to me to represent time as well as immensity of space. Here were miles of rippled purples, half transparent and of ineffable beauty. Now and then soft golden clouds floated from these folds, or a great shimmer went over the whole of these rolling purples, and things like green birds fell from it, fluttering down into the gulf below. Next I saw clusters of stones hanging down in masses from the claw toes : as it seemed to me miles of them down far below into the underworld of the black gulf.

3. From Havelock Ellis :¹

The visions never resembled familiar objects ; they were extremely definite, yet always novel ; they were constantly approaching, and yet always eluding the semblance of known things. I would see thick, glorious fields of jewels, solitary or clustered, sometimes brilliant and sparkling, sometimes with a dull rich glow. Then they would spring up into flowerlike shapes beneath my gaze, and then seem to turn into gorgeous butterfly forms or endless folds of glistening, fibrous wings of wonderful insects ; while sometimes I seemed to be gazing into a vast hollow revolving vessel, on whose concave mother-of-pearl surface the hues were swiftly changing. I was surprised, not only by the enormous profusion of the imagery presented to my gaze, but still more by its variety. Perpetually some totally new kind of effect would appear in the field of vision : sometimes there was swift movement, sometimes dull, sombre richness of colour, sometimes glitter

¹ Havelock Ellis, *op. cit.*

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and sparkle, once a startling rain of gold, which seemed to approach me. I was further impressed, not only by the brilliance, delicacy, and variety of the colours, but even more by their lovely and various textures—fibrous, woven, polished, glowing, dull, veined, semi-transparent.

4. From Knauer and Moloney :¹

High above me is a dome of the most beautiful mosaics, a vision of all that is most gorgeous and harmonious in colour. The prevailing tint is blue, but the multitude of shades, each of such wonderful individuality, make me feel that hitherto I have been totally ignorant of what the word colour really means. The colour is intensely blue, rich, deep, deep, deep, wonderfully deep blue. It is like the blue of the mosque of Omar in Jerusalem. . . . A beautiful palace, filled with rare tapestries, pictures, and Louis Quinze furniture has been peacefully unfolding itself, room after room, each a little different from all the rest, all marvellously bright and beautiful, and all coloured in the same scheme—violet, cream, and gold.

5. From Rouhier :²

It is night—water—yet more water—always with the reflection of the moon. It is wonderful to see. A great expanse of water on which a long galley is gliding—a young girl is standing on its prow. An unseen light is flooding it with brilliant mauve beams following the movement of the boat. The water is spattered with mauve reflections—the violet glimmer plays on the white sails of a fleet of accompanying galleys. . . .

The sombre porch of a church before which is standing an old woman in white. A monk comes out of the church followed by a young girl also in white. He carries a lantern which lights up all three. Their movements are slow and majestic. They each have an amazing personality. This little picture is inexpressible in depth of meaning and expression. It is an illuminated and living sculpture.

This subject subsequently described her experiences thus : that she had received 'an experience of unimaginable art, unforgettable, and of an intensity for which there are no words and which it is necessary to experience in order to understand'.

Changes may also be produced in other senses. Auditory hallucinations of wonderful music and voices speaking in strange languages have been reported but are rare. The perceived body may undergo the most extraordinary changes : parts may grow or shrivel, become

¹ A. Knauer and W. J. M. A. Maloney, 'A Preliminary note on the psychic action of mescaline', *J. Nerv. Ment. Dis.*, 1913, 40, 397

² A. Rouhier, *Le Peyotl*, Paris, 1927 (my own translation)

stone-heavy and cold, the perceived limbs may detach themselves from the body and lie on the floor beside the observer. This latter effect may strike an onlooker as being amusing since the subject's physical limbs are clearly in their proper place. This amusement does not survive the onlooker's own experience of this phenomenon. Touch and pressure sense may also be affected and hard objects may be felt to be soft and malleable. An irresistible desire then arises to engage in the novel experience of moulding stones and kneading the walls of the room. Fragrant perfumes may be smelt and curious tastes experienced. Finally, in the realm of sensation, the interesting phenomenon of synaesthesia may occur. Sounds may be accompanied by 'appropriate' visual imagery, as may emotions, but the process sometimes goes further than this and forms of perception intermediate between the various senses may be experienced. The subject may, for example, be quite unable to say whether his experience is visual or auditory. Schilder¹ believes that such synaesthesiae represent very primitive forms of sense perception.

The most complex changes take place within the ego. This may be felt to split noticeably into its subjective and objective components and states of derealisation or depersonalisation may be produced. The boundaries normally separating the ego from its environment may be eroded and in this way a psychotic state is induced. One of Beringer's subjects exclaimed:² 'I am fretwork; I hear what I am seeing; I think what I am smelling; everything is fretwork; . . . I am music; I am climbing in music; I am a touching fretwork; everything is the same.'

In the sphere of the emotions mescaline may produce a bubbling sense of well-being; a state of ecstasy; a state of what the subject may describe as the 'ending of all desire'; or a fearful state of terror and despair. Most commonly, as far as the emotions are concerned, a mild sense of well-being is all that is experienced.

From the point of view of physics, three of the most interesting effects of mescaline are those that it exerts on the subject's perception of space, time, and movement. Spatial relations in the visual field become grossly disordered. The perceived room may grow enormously in size or take on an entirely new shape. Rooms and houses

¹ P. Schilder, *Mind, Perception and Thought in their Constructive Aspects*, New York, 1938

² K. Beringer, taken from the translation by H. Klüver, *Mescal. The Divine Plant and its Psychological Effects*, London, 1928, p. 37

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become distorted after the manner of the sets in 'The Cabinet of Dr Caligari'. One of Beringer's subjects reported rather amusingly :¹ 'I looked out of the window and was particularly surprised at the changes in the size of the houses ; they seemed to have grown after the manner of skyscrapers. . . . The bread I held in my hand did not become smaller ; what I bit off at one end grew again at the other.'

Time sense often becomes distorted and its passage very much slowed down. The subject will judge the passage of a few minutes to be an hour. Actions do not appear to be carried out in slow motion but teatime goes on forever. The subject will feel quite literally that he is at the Mad Hatter's tea party.

The most interesting changes take place in the perception of movement. Ordinarily still objects may be seen to rock to and fro in a rhythmical manner, the walls to sway, and the angles of the room to become alternately acute and obtuse.

One of Beringer's subjects reported thus :²

I am sitting before a shelf which is attached to the wall and on which there are several bottles, empty glasses and small test tubes. The shelf has not its normal shape ; the boards are curved as though they were made of rubber. The whole shelf becomes alternately higher and lower ; now it is leaning towards me, now it retreats to the wall. Other objects in the field of vision take part in these movements. The bottles also behave as though they were made of rubber ; they fold up after the manner of opera hats ;

and again most interestingly :³

The perception of a moving burning cigarette was a great surprise to me. Not a continuous line or circle was seen, as under normal conditions in the dark room, but a number of small glowing balls. At the end of the movement I could see the entire movement as it were fixed by a number of glowing balls standing in the air. Then these balls jumped all of a sudden in a great hurry into the glowing end of the cigarette. They did not fade, but all of them went along the curve to the terminal point just as if they were connected by a rubber band. Everything was so distinct that I was able to count the glowing balls ; at one time I counted sixteen ; there was no luminous line between the glowing balls ; it was quite dark. . . . The faster the movement, the more the transition from balls into lines and a decrease in the distance between them. . . .

¹ From Klüver, *ibid.*, p. 70

² From Klüver, *ibid.*, p. 79

³ From Klüver, *ibid.*, pp. 74-75 and 78

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I saw that the scales of the fish as well as the fish itself were distinctly moving. [They were at dinner.] I was unable to eat it. I admired the certainty with which Dr B. was convinced of the death of the fish. The noodles behaved literally and without exaggeration as a moving heap of worms.

It must be emphasised that mescaline produces predominantly disorders of experience. It is the actual perceived events that undergo these startling changes. There is no question of 'imagining' these phenomena or of seeing things that are 'not there'. In the case of the disordered perception of world events the sense-data of experience quite blatantly cease to follow the detail of the events in the physical world they correspond to, and which the subject knows about since he has been told to expect, for example, the movement of a cigarette in a circle, the appearance of which he knows from memory. In the case of the visions, the appearance that they present to the observer is essentially the same as that presented by the visual field of his everyday experience. The words used by these subjects supports this contention. They use such phrases as 'I saw', 'As I gazed', 'As I looked', 'It is wonderful to see', etc. The visions differ only in their superior aesthetic quality and because they correspond to no object in the common physical world. These phenomena present the following problems. Why should a brain which cannot utilise glucose properly produce such interesting phenomena? How may these visions be related in their internal three-dimensional spatio-temporal structure to the electrical patterns in the cortex, which possess an entirely different spatio-temporal structure determined by the complex convoluted shape of the cortex? I have suggested elsewhere how these phenomena may actually be produced.¹

I will conclude this brief account by quoting the opinions of Klüver, Havelock Ellis, and Macdonald Critchley concerning the quality of these phenomena.

6. From Klüver :²

. . . it is true that the experiences in the mescal state are not easily forgotten. One looks 'beyond the horizon of the normal world', and this 'beyond' is often so impressive or even shocking that its after effects linger for years in one's memory.

¹ J. R. Smythies, Reply to comments by Professor H. H. Price and others, *J. Soc. Psychological Research*, January 1952, **36**, 557

² H. Klüver, *Mescal. The Divine Plant and its Psychological Effects*, London, 1928

THE MESCALINE PHENOMENA

7. From Havelock Ellis :¹

. . . a large part of its charm lies in the halo of beauty which it casts round the simplest and commonest things. . . . If it should ever chance that the consumption of mescal becomes a habit, the favourite poet of the mescal drinker will certainly be Wordsworth. Not only the general attitude of Wordsworth, but many of his most memorable poems and phrases cannot—one is almost tempted to say—be appreciated in their full significance by one who has never been under the influence of mescal.

8. From Macdonald Critchley :²

The usual emotional content of the hallucinations is best described as one of amazement, awe, interest, and delight. The character of the visions is such as to impress the most prosaic and unimaginative observer in a manner of which no natural beauty is capable. Almost all writers have insisted that the most skilful pen or brush could not do justice to the marvel of the hallucinations.

These remarks alone would, I suggest, render these phenomena worthy of the most careful consideration by philosophers.

I am very grateful to Edward Osborn and Humphrey Osmond for their most welcome help and advice.

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¹ Havelock Ellis, *op. cit.*

² Macdonald Critchley, 'Some forms of drug addiction. Mescalism', *Brit. J. Inebriety*, 1931, 28, 99

NOTES AND COMMENTS

Are There Quantum Jumps ?

PROFESSOR SCHRÖDINGER in two articles in the August and November (1952) issues of this *Journal* asks the question 'Are there quantum jumps?' and answers it in the negative. If his conclusion were generally accepted, much of the quantum theory would need to be rewritten. May I submit some considerations which seem to me to be relevant?

In the articles referred to, Professor Schrödinger deals with jumps of energy, and remarks that since energy is equal to frequency multiplied by Planck's quantum h , the jumps of energy may be regarded as being really transitions between discrete proper frequencies. It seems to me, however, that the strongest argument for quantum jumps is the fact that there are not only jumps of *energy* but also jumps of *momentum*. A striking case is the simple and beautiful quantum explanation of the formation of spectra by a grating, which is due to W. Duane.

Consider an infinite grating with the spacing d between the rulings. The grating, when irradiated by a parallel beam of light, is a periodic system with respect to a co-ordinate q where direction is in the plane of the grating perpendicular to the rulings, the period being d ; so if p denotes the component of momentum in this direction exchanged between the grating and the radiation, the Wilson-Summerfeld quantum condition gives

$$\int p \, dq = \text{a multiple of } h$$

or, since d is the period over which the integrations must be taken,

$$p \, d = \text{a multiple of } h.$$

that is, *the grating can pick up momentum p from the radiation only by quantum jumps*, namely in multiples of h/d : the momentum of radiation is transferred to matter in quanta. If a photon of frequency ν and therefore of momentum $h\nu/c$ falls on the grating in a direction making an angle i with the normal, and is diffracted in a direction making an angle r with the normal, then the equation of conservation of momentum in the p -direction is

$$(\sin i - \sin r) \cdot h\nu/c = nh/d,$$

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where n is a positive whole number. In the language of the wave-theory, this is

$$n\lambda = d(\sin i - \sin r)$$

which is *the ordinary equation of diffraction by a grating*.

I cannot think of any way in which Professor Schrödinger could re-state this argument in an equally simple form without making use of the conception of quantum jumps of momentum.

However, Professor Schrödinger may reply to all this, that if the performance of the grating is treated by the wave-theory of light, there is no need to have quantum jumps : and perhaps the proposition he is putting forward is that *in every physical problem there exists a wave-theory which enables the problem to be treated without using quantum jumps* : if this is his contention, then I do not dispute it : but, in my opinion, if corpuscular conceptions are retained at all, then quantum jumps are inevitable.

Incidentally, I might add, that this example of Duane's solution raises a question of nomenclature. The wave-theory of light was discovered before quanta were ever heard of, and, owing to this historical accident, it is usual to call the wave-theory the 'classical' theory. But waves of light are simply the de Broglie waves associated with the photons which figure in the corpuscular theory of light : and the wave-theory is therefore really a quantum theory : interference and diffraction are essentially quantum phenomena, as is shown by the fact that when they are treated by the corpuscular theory (Duane's method, as given above) they require quantum jumps, although when treated by wave-theory they do not require quantum jumps. Moreover, the interference and diffraction of light are evidently of the same nature as the interference and diffraction of electron-beams and of molecular rays, and the latter phenomena are undoubtedly quantum effects.

EDMUND WHITTAKER

Has a Single Electron a Transit Time ?

PROFESSOR SCHRÖDINGER¹ emphasises the significance of the frequencies, rather than the energies, of electronic states, and draws a distinction between macroscopic energy and microscopic energy. This reinforces my argument² calling for an experimental study of

¹ This *Journal*, 1952, 3, 109-23

² This *Journal*, 1952, 3, 243

the conditions of breakdown of the classical concept of electron velocity (and hence of kinetic energy), for example by determining the transit time of a single pulse consisting of a very few electrons without using periodic fields. Is there a finite probability of some electrons arriving before they should, even allowing for quantum indeterminacy? Is there a head wave of higher velocity?¹

L. L. WHYTE

The General Adaptation Syndrome in the Study of Animal Populations

MR LE VAY has pointed out, in a recent number of this *Journal*, the importance of Selye's unifying concept of the General Adaptation Syndrome and the Diseases of Adaptation.² In an experimental study of populations of the vole (*Microtus agrestis*) at the Bureau of Animal Population, Oxford, Selye's concept has been of considerable value.

The subject of mammalian population dynamics has been extensively reviewed by Elton.³ At the time at which Elton wrote, no single primary cause for these population fluctuations could be found, although Elton had mentioned the possibility that fighting among voles might be important in determining their numbers. Errington,⁴ writing about mammals in America, had made similar suggestions. The question of the rôle of fighting amongst members of the same species, and its importance in determining their numbers, was made clearer by a hypothesis put forward to explain population cycles in the vole,⁵ which may be summarised in this way. As the numbers of voles, constituting a natural population, rises, fighting between them was suggested as increasing. Apart from mortality arising directly from such fighting, it was postulated that this strife causes considerable physiological disturbance of the participating female animals. Further, it was suggested that such disturbances, when occurring in pregnant female voles, gives rise to abnormalities in their young. Because of these changes the population declines, or 'crashes', below optimal densities, later recovering only to crash again once the peak densities are reached.

¹ This formulation is due to Dr D. Gabor.

² H. Selye, *The Physiology and Pathology of Exposure to Stress*, Montreal, 1950

³ C. Elton, *Voles, Mice and Lemmings. Problems in Population Dynamics*, Oxford, 1942

⁴ P. L. Errington, *Quart. Rev. Biol.*, 1946, **21**, 144, 221

⁵ D. H. Chitty, *Phil. Trans. Roy. Soc., B*, 1952, **236**, 505

THE GENERAL ADAPTATION SYNDROME

It was known that voles fight with extraordinary ferocity in the laboratory. The first step, then, in testing this hypothesis under laboratory conditions, was to show that fighting could produce the physiological disturbance suggested as occurring in the combatant voles. The difficulty was to find a suitable index. The laboratory in which this research was going on was equipped for ecological and not for physiological research, so that, however desirable, a detailed study of the form and function of a number of organs of the voles was not practicable. Even had such a study been possible, no significance, as far as this hypothesis about population dynamics was concerned, could have been read into the results. What was required was a quick, uncomplicated measure of the physiological state of the animals, which would be at once meaningful as far as the hypothesis was concerned.

It was here that Selye's concept of the General Adaptation Syndrome proved invaluable. As Selye regarded the enlargement of the adrenal glands and the involution of the thymus as typical both of the diseases of adaptation and of the reaction of an organism to stress, and as fighting in voles, with its vigorous chasing and wounding, seemed to be an effective stressing agent, it seemed advisable to use the weight of the adrenals and of the thymus as an index of the physiological state of the voles.

By simply weighing on a torsion balance the adrenals and thymus of voles which have been fighting, and comparing their weights with those of the control animals living alone, it has been shown that fighting causes a marked increase in the weight of the adrenals, and a decrease in the weight of the thymus. Although there has been no parallel histological study, it seems reasonable to assume that these weight-changes reflect the sort of physiological disturbance which Selye and others have shown to occur in other animals as a response to certain other stressing agents.¹ Had Selye not put forward his hypothesis concerning disease processes, then this present attempt to test an ecological hypothesis would not have proceeded even thus far.

Though it has been possible to show, with some degree of certainty, that the voles which actually fight suffer changes in part of their endocrine system, it is proving much more difficult to produce the abnormalities of the young which the hypothesis requires. This

¹J. R. Clarke, *J. Endocrinol.*, 1952, in the press

JOHN R. CLARKE

is partly because, so far as is known, no clear-cut mechanism for changes of this sort has to date been proposed by physiologists. It is difficult, therefore, to know what facts to collect, and how to interpret any that are obtained.

It is seen, then, that a study of population dynamics has been considerably helped by the investigations of physiologists. More recently the methods used in this ecological investigation of subjecting animals to the stress of fighting has proved useful in a physiological study.¹

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JOHN R. CLARKE

Mind-Like Behaviour in Artefacts

DR MAYS' comments (this *Journal*, 1952, 3, 192) on my recent paper (vol. 2, p. 105) reveal a rather odd misunderstanding. Looking, apparently, for an implication 'that our decisions may ultimately be explainable in terms of physics and physiology' he quotes me correctly as saying: 'The suggestion is that the choices of a free man may likewise be governed by statistical distribution-functions which have a physiological representation and are in principle determinate . . .' He omits, however, the conclusion of the sentence: ' . . . but that individual choices can be unpredictable in principle, and it is probable that the distribution-functions are indeterminable in practice'. Mr Mays' misunderstanding of what is meant by a distribution-function may be excused; his excision of that half of the sentence which should have prevented it is more puzzling.

The important distinction between the two sub-senses of 'random' mentioned by Mr Mays is familiar to information-theorists, who distinguish between statistically 'random' code-signals and 'noise' in a similar way. But surely Mr Mays does not think of motivating factors as 'purely random' in either sense? In splitting the specification of the artefact's activity into systematic and random components I was using a common artifice of statistical physics, by which one thinks of any pattern (e.g. a radio signal), however chaotic, as equivalent to some combination of 'ordered patterns' perturbed by 'purely random fluctuations'. Inasmuch as motivating factors are

¹ W. S. Bullough, *J. Endocrinol.*, 1952, 8, 265

THE MECHANICAL CHESS-PLAYER

not (in my experience, nor, I notice, in Dr Pirenne's (vol. 2, p. 315)) equivalent to mental coin-tossing, they of course form part of the systematic component, and are far from 'left out of account'.

It is less certain that a purely systematic process determines *which* motivating factors operate in the making of a given decision; and a measure of perturbation at this level would I think be quite compatible with one's experience. The organisation of the model I have in mind¹ is hierarchical, and has room for a measure of indeterminacy at each level. My intention in the brief paragraph on this topic was merely to draw attention to the intriguing role that can be played by such 'purely random' processes, as the origin of intelligible and fundamental novelty in its activity.

My three sentences on human choice I now regret, not indeed because they seem any less valid, but because I consider unpredictability to have less importance than other criteria for determining responsibility, and I think the emphasis laid by omitting to say so was wrong. Fascinating though the implications of unpredictability are for moral philosophy, I am convinced, as I have pointed out elsewhere,² that it does not offer the proper solution to the apparent problem of free will.

D. M. MacKAY

The Mechanical Chess-Player

MAY I be permitted to comment on Dr W. R. Ashby's discussion of the mechanical chess-player and on Professor J. B. S. Haldane's note on the same? The precise status of information (or specification, control, design, etc.) deserves, I think, to be expressed rather more clearly, especially with reference to message and noise. Consider, for example, the following two statements:

. . . the designer must construct the machine so that it can receive and use information not provided by him in detail. The extra information need not be organised or intelligible—that given by thermal noise may be sufficient. (Ashby, *this Journal*, 1952, 3, 57.)

. . . natural selection . . . (is) a method for the conversion of noise into message. (Haldane, *ibid.*, p. 190.)

¹ Further explored in *Proc. Arist. Soc. Suppl.* 1952, pp. 61–86.

² 'An artefact's approximation to voluntary behaviour', paper read before the Philosophy of Science Group on October 13, 1952, to be published later; also 'From Mechanism to Mind', *Trans. Vict. Inst.*, forthcoming.

It is possible that Haldane has private definitions of 'noise' and 'message' that justify his remark. But Ashby goes so far as to use the term 'thermal noise', by which we must surely understand a process as absolutely and irresolubly random as is physically possible. The implication in both quotations is that information (in the sense of regularities, uniformities, 'laws of nature' and the like) can somehow be extracted from a random process by applying a predetermined rule. It will be remembered, however, that the essence of the von Mises Irregular Collective is that a sample drawn by any systematic rule whatever, tends, in the limit, to have the same frequency distribution of symbols as the parent collective.

In actual fact, Ashby only invokes noise as a convenient means of ensuring that, given time, every permitted variation of the system will (or can) appear. It is a very inefficient method by everyday standards, since it *may* go on repeating useless variations indefinitely. But where no better mechanism seems practicable or discoverable, as in evolution, we are forced to accept it. Ashby appears to think that neither evolution nor learning can go forward unless the mutations are random; I would say that a systematic search of the mutation group would be better, but a random one will do. But to regard noise, used for this purpose, as information is misleading. It is much more akin to power; indeed, in Ashby's homeostat the scanning operation could be much better performed by a simple rotary switch. There is no significance in the random numbers he has inserted.

Again, when Haldane speaks of the 'conversion of noise into message', he must either be sacrificing precision for brevity or be using the words in other than their usual senses. For if there is one thing you cannot do with noise, it is to get anything that could be called a message out of it; the two are by definition antithetical. It is true that what appears at first to be noise may be found to conceal a message, in the sense of regularities which, when known, permit predictions of greater or less accuracy. But in that case the phenomenon is not pure noise; its entropy is less than the maximum, on account of the redundancy. It is also true that one may require to transmit noise as a message, merely because one happens to want a random process somewhere else than the place at which it is generated. But this is a matter of definition, and natural selection has nothing to do with it—except in so far as it may be responsible for the existence of beings capable of talking about such things. All that natural selection can do with noise is what the experimenter does

COMMENTS ON EPIPHENOMENALISM

with random errors—repeat the experiment so many times that the errors largely cancel out and reveal the constancies. The process is not one of conversion but of separation.

I am not sure that it is fully realised that design (or specification, etc.) is, like beauty, in the eye of the beholder. The amount of specification in a system is the extent to which we can in fact specify it. A complex molecule contains no more intrinsic design than a random collection of its component atoms, unless and until we know how its atoms are put together. If only we could persuade the particular random arrangement to stay still long enough (or recur often enough) we could specify that too ; but then we should stop calling it random. Randomness is only the projection of ignorance onto the external world. To say, as Haldane does, that Brahma might create intelligent beings by setting up favourable conditions and then waiting for chance to do the rest is merely to say that Brahma would not be entirely responsible. He would be relying on events that he could neither predict nor control, and his experiment might fail—his dreams go awry. It is pretty, but I do not see that it is relevant to the question of chance and design in cosmology. We see design in the universe because we would not be here if we did not ; so, at least, we read in that bit of the design we call 'science'. But that is scarcely 'information', for any other answer we should reject as unscientific. The logons, as Gabor and MacKay would say, have been settled beforehand.

W. E. HICK

Comments on the Alleged Proof of Epiphenomenalism

In a recent article on 'Apparent Motion and the Mind-Body Problem',¹ Dr George Watson contends that epiphenomenalism is the only current psychological theory compatible with present experimental knowledge. Since this thesis implies what Lovejoy has aptly referred to as the 'sterility' of consciousness, its proof would entail very considerable consequences ; and it therefore calls for some comment.² The empirical evidence offered is the phenomenon of illusory

¹ This *Journal*, 1951, 2, 236-247

² In the realm of speculative and moral philosophy the empirical proof of epiphenomenalism would be as momentous as the introduction of relativity and quantum theory into physics.

perception of motion, from which the sweeping generalisation is derived that 'There is a structural, qualitative, and spatio-temporal diversity between neuroevents and psychoevents, the latter succeeding in time and being causally dependent upon the former' (p. 247). In essence the author's argument consists of the following propositions: (1) Perception is a typical instance of all neuro-mental events; (2) There is a single neural event mediating between physical stimulus and correlated consciousness; (3) All psycho-physical theories, other than epiphenomenalism and perhaps interactionism, are incompatible with the facts of illusory perception of motion; (4) Interactionism is either incompatible with these facts or is discredited for other reasons; (5) Epiphenomenalism is compatible with the facts.¹ I wish briefly to argue that each of these propositions is controvertible and that consequently the author's thesis has not been proved.

(1) The author's argument turns on the assumption that the phi phenomenon is a representative instance of the mind's relation to its organic and physical environment. It is a crucial question, however, whether we are entitled to assume that communication with the past and with the future are identical in principle. In perception the mind is patient to a series of antecedent events terminating in consciousness; but in purposeful control of the future, whether by change of attitude, of body, or of the larger environment, it seems to be a dynamic or independent variable.² To meet this difficulty an appeal is made to such data as startle, impulse, and emotion to show that in action consciousness is the temporal terminus of a series of bodily events antecedent to it. But facts of this type scarcely pre-empt the field of selective response or purpose; and unless they do the author's evidence is irrelevant to the generalisation that he has proposed.

(2) The central and most impressive argument of the paper, that extra-epiphenomenalistic theories are incompatible with the significant 'diversity' in time and structure between neuroevents and psychoevents, rests on the implicit assumption that a single neuroevent

¹ The author's assumption will be disregarded, that the environing physical order of consciousness is in absolute, Newtonian time.

² In perception, of course, the mind cannot arbitrarily be asserted to remain a passive spectator. The apprehension of motion, for instance, involves an implicit judgment of the identity of two objects in different space-time; and in the phi phenomenon, at least, this judgment is false.

mediates between physical stimulus and conscious perception. This may be represented as $S-N-P$. But it has not been shown that two neuroevents are not involved, or may be involved, in an instance of perception, one correlated with the stimulus and the other with the psychoevent. This may be represented as $S-N^1-N^2-P$. The evidence, indeed, suggests that this latter interpretation is a plausible and useful hypothesis; and if it can stand, the alleged advantage of epiphenomenalism dissolves.

(3) Assuming, however, that a single neural event mediates between the physical and the mental correlates it is questionable whether theories other than interactionism and epiphenomenalism must be ruled out. On the double-aspect and psychical monist theories it could be maintained that the neural concomitant of the physical stimulus is in turn correlated with 'pure' experience in the subliminal (marginal, subconscious) level of the mind; in which case the displacement or rearrangement of objects and structures in perceptual consciousness, and the false imputation of motion, would comprise a second mental event associated with its own neural correlate. Objections against parallelism are valid only if it be assumed that this theory requires simultaneity and perfect isomorphism of structure between neuroevent and psychoevent. This assumption, however, is gratuitous. If parallelism is understood to mean objective correlation¹ of two events without causal dependence or interaction, 'parallel' neuroevent and psychoevent may occur in different times and involve non-isomorphic structures. This hypothesis, indeed, would seem to hold a decided advantage over epiphenomenalism; since whereas the latter appeals to a metaphysical concept of 'causal dependence', the former is susceptible of various interpretations and as a scientific hypothesis can remain speculatively neutral.

(4) Interactionism, which posits mind as an active variable, with its own attributes and laws, seems quite compatible with the facts of illusory perception, and is in many respects the most convenient of all psycho-physical theories. To meet the force of this consideration the author attempts to shift the burden of proof. It is demanded that those who defend idealism or interactionism 'make good' their claim (p. 244). They must 'show that in volition there are no [completely determining neural] causal antecedents' (p. 245). This, however, is to be denied. The traditional view of the fertility of

¹ 'Objective', i.e. grounded in a principle of connection other than chance.

consciousness is too well supported by practical or presumptive evidence¹ to permit such a transfer of the *onus probandi*. The epiphenomenalist, I submit, is required to present overwhelming evidence that the commonsense view is mistaken, at least when he speaks dogmatically. It is incumbent upon him to show that in every type of apparent control of the future by the mind the neural antecedents and concomitants are both its necessary and sufficient condition.

(5) The author's proof of epiphenomenalism seems to rest on the argument of elimination of alternative theories. No attempt has been made to show that this theory is either credible in itself or compatible with the anomalous facts of illusory perception. As employed in the paper under consideration, however, it involves thorny difficulties, some of which are briefly as follows: (a) 'Causal dependence' is not defined by the author. If this term means invariable temporal antecedence of neuroevents to psychoevents, it is simply a special case of parallelism. If it means something more, we move in the direction of metaphysical obscurity. (b) It has not been shown precisely how the notion of 'causal dependence' clarifies the puzzle involved in the 'structural, qualitative, and spatio-temporal diversity' between neuroevents and psychoevents; and I do not see what advantage can be derived from its employment. The difficulty remains whether the relation be termed 'parallel' or 'causal'. (c) On the epiphenomenalist theory the origin of the concept of 'causal action' is an embarrassing problem. It cannot be derived from an intuition of activity or responsibility in the mind, since that theory denies mental activity. Nor is it likely that the author would care to resort to an *a priori* category or innate idea. (d) Perhaps the *bête noire* of epiphenomenalism, however, is the question as to how personal and logical identity can be explained in terms of neural similarity. No two instances of neural events, whether electrical or chemical, are identical; and yet, according to the theory, they explain the fact that two instances of personal consciousness can involve an acknowledgment of self-identity, and two thoughts about the term 'neuron' can refer to a single logical meaning. The rapid physiological replacement of chemical components of the nervous system, add serious difficulties.

WILBUR LONG

¹ Moral evidence, e.g. supporting belief that other minds exist, that the environment of consciousness is a realistic world, that a past and present self may be identical, that a person is a morally responsible agent.

THE FUNCTION OF CHEMICAL FORMULAE

Time of Psychology and of Physics

ON re-reading my criticism of Dr Dobb's papers in your May issue,¹ I see that I have been guilty of a misleading statement which I hasten to correct.

I wrote: 'I cannot agree with Dr Dobbs that Milne's τ and t times have anything to do with the dimensional problem, nor even that there is an "obvious similarity".' So far correct, but I continued: 'These two time-scales refer to two isoperiodic systems within the same dimension *and are essentially comparable, in the spatial field, to a standard meter and a standard yard, except, of course, that the difference between the two scales is very small.*'

The words now italicised are misleading. The analogy is sound in so far as that it is true that the two time-scales are within the same dimension, that we can arbitrarily choose to use either scale (or some other) and that the one can be simply converted into the other. But, as Milne himself has put it in his posthumous book²: 'We pass from one method of measuring time to another which differs from it far more fundamentally than by a mere change of units and of zero'.

The use of the unfortunate analogy in no way invalidates the rest of my argument, but I should not wish it to remain uncorrected.

G. W. SCOTT BLAIR

On the Function of Chemical Formulae :

A Nineteenth-Century Discussion

READERS of this *Journal* may be interested in the views on the function of chemical formulae which were put forward in the forties of last century by Auguste Laurent³ and Charles Gerhardt,⁴ possibly the greatest chemists of their epoch.

Given the somewhat chaotic condition of chemistry at that time, it is impossible within the limits of space which I have set myself to sketch with all the desired accuracy the context in which these views

¹ G. W. Scott Blair, this *Journal*, 1952, 3, 82

² E. A. Milne, *Modern Cosmology and the Christian Idea of God*, Oxford, 1952, p. 95

³ Owen Potter, 'The Contributions to Chemistry of Auguste Laurent', *Annals of Science* (to be published)

⁴ E. Grimaux et C. Gerhardt, *Charles Gerhardt, sa vie, son oeuvre, sa correspondance*, Paris, 1900

found expression. Briefly, one may recall the uncertain state of the atomic theory, though its concepts were used, and 'abused', by all chemists.

Although the discoveries of isomorphism and isomerism were immediately brought into relation with the arrangement of the atoms—as by Berzelius when of isomerism he wrote, 'it would seem as if the simple atoms of which substances are composed may be united with each other in different ways'¹—scepticism tended to prevail when consideration was given to the question of deciding on the positions of the atoms in the molecule. Auguste Comte summed up this attitude when he wrote: 'the real mode of agglomeration of elementary particles is, and ever must be, unknown to us, and therefore no proper object of our study'.²

In so much as the 'obvious' rôle of chemical formulae was to represent in some way the positions held by the various atoms in the molecule, the above view led to some confusion.

Charles Gerhardt, in his *Précis de chimie organique* of 1844-5, attacked the 'rational formulae' of Berzelius, arguing that 'empirical formulae' alone were of importance. He even attempted a classification of organic compounds based on empirical formulae. In a then unpublished criticism of this work, Laurent singled out for attack Gerhardt's emphasis on empirical formulae.

Dans sa classification, M. Gerhardt laisse de côté les formules rationnelles ! Sans nier qu'il y ait dans les corps composés un certain arrangement dans les atomes, arrangement d'où dépendent les propriétés des composés, il prétend que nous ne saurons jamais rien sur ce sujet, que par conséquent tout système basé sur des formules rationnelles sera toujours vicieux. Nous ne partageons pas l'opinion de M. Gerhardt, nous croyons à l'existence du groupe *ammonium*, nous croyons que dans les sulfate, nitrate, acétate de morphine, il y a un groupement morphinique. . .³

During the ensuing years, Laurent did not let pass any opportunity to persuade his colleague to give up the advocacy of 'brute' empirical formulae. Meanwhile he himself, doubtless under the pressure of Gerhardt's criticism, adopted what he called 'synoptic formulae'.

¹ *Jahres-Bericht*, 1832, p. 44

² I. A. M. Comte, *Cours de philosophie positive*, Paris, 1830-42, vol. 3, 115

³ M. Tiffeneau (editor), *Correspondance de Charles Gerhardt*, Paris, 1918, p. 276

THE FUNCTION OF CHEMICAL FORMULAE

J'emploie un système particulière de formules que j'appellerai *synoptiques*. Elles ont, je pense, les avantages des formules brutes et rationnelles sans en avoir les inconvénients. Je n'ai pas le prétention de représenter par mes formules l'arrangement réel des atomes. . . . Ce sont bien des idées sur l'arrangement moléculaire qui m'ont guidé dans le système que j'expose ; mais on peut, si l'on veut, en faire abstraction, et ne voir dans mes formules que des symboles dont l'aspect rappelle à l'instant même, non-seulement la composition et la nature du corps qu'il représentent, mais encore la série à laquelle ce corps appartient et la place qu'il doit occuper dans cette série.¹

Classification was always in the forefront of Laurent's mind, and it is not surprising then that he should come to regard the function of a formula as indicating the series of compounds to which a body belongs and the place in this series which the body occupies : 'à un même genre de corps correspond une formule semblable'.² In a letter to Gerhardt, Laurent expanded these views.

Puisque nous ne prétendons pas que l'arrangement [of atoms] est indiqué par les formules, nous voulons seulement dire : si l'acide acétique = $C^8H^8 + O^4$, le formique sera $C^4H^4 + O^4$

On est donc à l'abri de la critique, on ne fait pas d'hypothèse.³

The year 1848 saw the publication of Gerhardt's *Introduction à l'étude de chimie par le système unitaire*. In a resumé, Gerhardt pointed out :

Dans le système dualistique [of J. J. Berzelius] on définit les corps *en tant qu'ils existent* ; cette méthode est incompatible avec notre système. Celui-ci prend ses définitions dans les métamorphoses des corps, c'est-à-dire *dans leur passé ou dans leur avenir* et, sous ce rapport, il répond évidemment au but spécial de la chimie, qui est la recherche des lois des transformations de la matière.⁴

The meaning of this passage becomes more evident when one reads his *Traité*, quoted below.

In Laurent's *Méthode de chimie*, published posthumously in 1854, his position remains unaltered as is shown by the following quotations.

In common with the majority of chemists, I admit that there is a certain predisposition in the arrangement of the atoms, but

¹ *Comptes Rendus*, 1844, 19, 1098

² *Rev. Sci.*, 1844, 3, 147

³ *Correspondance de Charles Gerhardt*, op. cit., p. 41

⁴ *Comptes Rendus mensuels des travaux chimiques par MM. Aug. Laurent et Ch. Gerhardt*, 1848, 208

distinctively from them, I believe that it is impossible for us to know this arrangement, but that we may nevertheless ascertain, whether in any one particular body, it is the same as in some other body.¹

I employ synoptical formulae as a means of indicating, not the arrangement of the atoms, but simply the analogies existing between certain classes of bodies. I acknowledge, in common with the rest of the world, that the ammoniacal nitrate is a nitrate ; consequently, if I represent nitrate of potash by $K.NO^3$, I must give to nitrate of ammonia a similar formula, namely $NH^4.NO^3$ ²

I employ *synoptical formulae* by the aid of which I endeavour to manifest certain numerical relations presented by seriated bodies, and to give to analogous bodies, analogous formulae. . . . Be the formulae that I employ what it may, for example that of the diamides, it might be considered as an hypothesis, for it is not demonstrated to the satisfaction of everyone, that this ether really is a diamide.

But on attentive examination it will be seen, that we have not to ascertain, whether in this ether there is such or such an atomic arrangement, but simply to determine, whether it has the properties of a salt or a diamide. Here then we are in the land of experiment.³

Gerhardt expounded his final doctrine in his *Traité*. His point of view may be summed up by the following quotations :

En résumé, les formules chimiques n'expriment et ne peuvent exprimer que des rapports, des analogies ; les meilleures sont celles qui rendent sensibles le plus de rapports, le plus d'analogies.

Ce caractère des formules chimiques rend évidemment oiseuses toutes les discussions qui portent uniquement sur la question de savoir *sous quelle forme* est engagé dans une combinaison tel élément ou tel groupe d'éléments, qu'on peut en extraire ou qu'on y a fait entrer si l'on n'attache pas à cette forme une idée précise de réactions ou de propriétés chimiques. Je conçois qu'on dise de certains corps azotés qu'ils renferment l'azote sous forme de vapeur nitreuse NO^2 , pour faire entendre que l'azote y a été introduit par l'acide nitrique, qu'ils font explosion par la chaleur comme les nitrates, qu'ils se réduisent par l'hydrogène sulfuré, etc. ; je conçois encore qu'on distingue deux isomères, comme l'éther méthyl-acétique et l'éther éthyl-formique en disant que l'un renferme le carbone et l'hydrogène sous forme de méthyle et d'acétyle, l'autre contenant les mêmes éléments sous forme d'éthyle et de formyle, pour indiquer ainsi qu'en plaçant ces deux corps sous l'influence du même réactif, on obtient avec l'un de l'ésprit

¹ *Chemical Method*, London, 1855, p. 324

² *ibid.*, p. 284

³ *ibid.*, p. 348

THE FUNCTION OF CHEMICAL FORMULAE

de bois et de l'acide acétique, avec l'autre de l'esprit de vin et de l'acide formique.¹

Les formules chimiques . . . ne sont pas destinées à représenter l'arrangement réel des atomes ; mais elles ont pour but de rendre évidentes, de la manière la plus simple et la plus exacte, les relations qui rattachent les corps entre eux sous le rapport des transformations.

Toute transformation, toute réaction chimique peut se rendre par une *équation* entre les matières réagissantes et les produits de la réaction. Représenter un corps par une *formule rationnelle*, c'est résumer par des signes de convention un certain nombre d'équations dans lesquelles figure ce corps, un autre corps étant pris, pour unité de comparaison. Les formules rationnelles sont donc en quelque sorte des équations contractées.²

Lorsqu'une semblable matière est mise en présence de différents agents capables de lui faire subir la double décomposition, il arrive souvent qu'elle ne leur présente pas à chacun le même côté pour l'attaque ; la double décomposition peut alors s'effectuer dans des sens différents. Une matière organique qui se comporte ainsi peut donc être représentée par plusieurs formules rationnelles.³

The apparent differences between the views of Laurent and Gerhardt are at least partially removed when one takes into consideration the former's statement that ' . . . a good classification ought to be, as it were, the resultant of all the reactions '.⁴

Those who see in the rôles thus assigned to chemical formulae the influence of positivism would doubtless like to have before them Comte's statement of the aim of chemistry.

. . . étant données les propriétés caractéristique des substances, simples ou composées, placées en relation chimique dans des circonstances bien définies, de déterminer exactement en quoi consistera leur action, et quelles seront les principales propriétés des nouveaux produits.⁵

To this Gerhardt's doctrine shows certain affiliations. But I do not know of any direct reference to Comte in the writings of either Laurent or Gerhardt.

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¹ *Traité de chimie organique*, Paris, 1853-6, tom 4, 563

² *ibid.*, p. 566

³ *ibid.*, p. 577

⁴ *Chemical Method*, *op. cit.*, p. 167

⁵ *Cours de phil. pos.*, *op. cit.*, tom 3, 20

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THE DIAGONAL METHOD IN FORMALIZED ARITHMETIC

1. The book ¹ deals with extensions of the very elegant formal system (S), which is a formalization of arithmetic in the sense of paragraph 2 below. This system is described in Chapter 2 (paragraphs 1, 5 and 6). There, rules are given stating what symbols are to be used, what sequences of symbols are to be called *well formed expressions* or *formulae*, and what sequences of expressions are to be called (S)-proofs. The rules are such that one can decide systematically whether or not a given sequence of expressions is an (S)-proof. This is not true, in general, of *R-proofs* which are introduced later on. For the convenience of readers the symbols of (S) are given here.

Symbols of (S)

Free variables	$a_1, a_2, \dots$
Bound variables	$\nu_1, \nu_2, \dots$
Arithmetic constants	$1, +, \times, =, <$
Propositional connective	$\rightarrow$
Operator	$(\mu\nu)$
Brackets	

Abbreviations (or explicit definitions)

$\rightarrow\Phi$	for $\Phi \rightarrow 1 = 1 + 1$
$\Phi_v \Psi$	for $(\rightarrow\Phi) \rightarrow \Psi$
$\Phi \& \Psi$	for $\rightarrow(\rightarrow\Phi_v \rightarrow \Psi)$
$\Phi \longleftrightarrow \Psi$	for $(\Phi \rightarrow \Psi) \& (\Psi \rightarrow \Phi)$
$(Ev)\Phi(v)$	for $\Phi((\mu\nu)\Phi(v))$ under certain conditions on $\Phi(v)$
$(Av)\Phi(v)$	for $\rightarrow(Ev)\rightarrow\Phi(v)$.

n denotes the n th expression in the sequence $1, 1 + 1, (1 + 1) + 1, \dots$

2. The rules for forming expressions, in particular, *sentences*,² and those for forming (S)-proofs are chosen in such a way that (S) constitutes a schematic model of our usual reasoning in arithmetic (and elementary analysis) when used as follows: the symbol ' $+$ ' is read as *plus*, ' $\times$ ' as *times*, ' $=$ ' as *is equal to*, ' $<$ ' as *is less than*, ' $\rightarrow$ ' as *implies*, ' $(\mu\nu)A(v)$ ' as *the least v such that $A(v)$ holds, or 1 if there is no such v* . With this interpre-

¹ A. Mostowski, *Sentences Undecidable in Formalized Arithmetic: An Exposition of the Theory of Kurt Gödel* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Co., Amsterdam, 1952, Pp. 117

² Sentences do not contain free variables.

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tation, a sequence of expressions which is an (S)-proof becomes more or less an informal argument of arithmetic, provided that the conjunctions *hence*, *since*, *now*, etc. are introduced judiciously. Conversely, after some experience one gets the knack of *formalizing* in (S) many of the usual text book arguments; that is, given such an argument one produces an (S)-proof which, on interpretation, becomes 'something like' the argument from which one started. The differences vary, the most trivial kind being that the words 'from *A* follows *B*' in the original argument will be changed to '*A* implies *B*'.

Now, there are many applications of ordinary arithmetic (school arithmetic or the calculus, its sharply differentiated extension) where the flexibility of ordinary language is no virtue, and a *precise concept of proof* is called for: for example when the problem is to show that a theorem proved in one way, say by the methods of analysis, cannot be proved in some other way, say by an *elementary* proof. In these applications formal systems are useful.

It is not reasonable to ask without qualification whether a formal system is a *correct* formalization of some given branch of mathematics, because the features which are to be considered characteristic of, or essential to, a branch are determined by the applications to which it happens to be put. A detailed discussion of this question seems to me worth while, but I shall not raise it again in this review. If a reader finds that the question obtrudes itself on him, he may consider whether particle mechanics, which characterises extended objects by their mass and centre of gravity, is a *correct* description of the solar system; or, again, if one says that such and such an argument cannot be formalized in (S), he should recall that the effects of rotations cannot be described by particle mechanics.

Incidentally, the author is very much interested in the relation between formal systems and informal, or, as he calls it, intuitive arithmetic. But he does not argue a case, and I shall not try to discuss his views.

3. Since the late nineteenth century several branches of mathematics have been formalized, among them propositional logic (a kind of baby mathematics) by Frege, the theory of classes by Boole, Frege, Russell and Whitehead, Euclidean and other geometries by Hilbert, arithmetic with ' $+$ ' as its only function symbol (that is, without multiplication) by Herbrand and Presburger. Hilbert has also given several formal systems for arithmetic.

Several of these formal systems were shown to be *consistent* and *complete* after Hilbert had laid special stress on these properties of formal systems. A system is called consistent if at most one of the two expressions *A* and $\rightarrow A$ can be proved in the system, and complete if of two sentences *A* and $\rightarrow A$ at least one can be proved. (The restriction to *sentences*, that is, expressions without free variables, is necessary; for, for example, $a = 1$ is

false if $1 + 1$ is substituted for a , and $a \neq 1$ is false if 1 is substituted for a .) Hilbert also defined the concepts of consistency and completeness for systems without a symbol for negation, but this is not important for the present review.

These concepts have a very general interest, not only in the formalization of a branch of mathematics, but also in applications of mathematics to physical theories : (i) a system of proofs classifies sentences into provable and non-provable ones ; if the system is inconsistent the classification is trivial since every sentence can be proved ; (ii) if the system is complete, then it is at least as good for classifying sentences as the informal branch which it is intended to replace, that is in the latter no proposition can be established whose counterpart cannot be proved in the system. Naturally, one does not realise the *mathematical* interest of these concepts without detailed study, that is, without examining general properties of systems which follow from their consistency or completeness. (Gödel's famous paper¹, which is the starting point of the book discussed in the present review, constitutes one of the most important advances in this study.)

In 1927 von Neumann² published a consistency proof of a formal system of arithmetic without induction (the first correct one of its kind, I think). (*Post factum*, that is after Gödel's arithmetisation, it is clear that von Neumann's argument could be formalized in (S), and even in primitive recursive arithmetic without quantifiers). The general tenor of the paper was this :

- (i) he expected, but did not claim, that some simple development of his argument should also establish the consistency of (S) ;
- (ii) though he did not expect (S) to be complete he observed that there was no clue whatsoever to a method by means of which the incompleteness (or completeness) might be established.

Both these points were settled by Gödel for a very wide class of formal systems. The details given in the book under review are discussed later on.

4. Here is a sketch of Gödel's methods. Numbers are given to expressions and sequences of expressions of the formal system concerned ; if the rules for forming expressions are sufficiently definite, and number theory can be 'developed' in the system, there is an expression $\sigma(a_1)$ such that $\sigma(n) = m$ can be proved in the system if and only if m is the number of the expression obtained when n is substituted for the free variable a_1 in the expression with number n . (This sentence is long, but not recondite.) $\sigma(a_1)$ represents Gödel's substitution function.

¹ K. Gödel, 'Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I', *Monatshefte für Mathematik und Physik*, 1932, 38

² J. von Neumann, 'Zur Hilbertschen Beweistheorie', *Mathematische Zeitschrift*, 1927, 26

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Now consider a property or class $\mathfrak{P}$ of expressions such that an expression N (that is the one with number n) is in $\mathfrak{P}$ *if and only if* the expression $P(n)$ can be proved in the system, and let $\mathfrak{R}$ denote the class of provable expressions. Apply Cantor's diagonal method, that is consider the expression $\rightarrow P(\sigma(p))$ whose number is, say, p . Then the number of $\rightarrow P(\sigma(p))$ is represented by $\sigma(p)$. Remembering the relation between $\mathfrak{P}$ and $\mathfrak{R}$ we have :

either $\rightarrow P(\sigma(p))$ in $\mathfrak{P}$ and $P(\sigma(p))$ in $\mathfrak{R}$;
or $\rightarrow P(\sigma(p))$ not in $\mathfrak{P}$ and $P(\sigma(p))$ not in $\mathfrak{R}$.

If now $\mathfrak{R} \subseteq \mathfrak{P}$, that is every provable expression has the property $\mathfrak{P}$, we have :

either $\rightarrow P(\sigma(p))$ in $\mathfrak{P}$ and $P(\sigma(p))$ in $\mathfrak{P}$;
or $\rightarrow P(\sigma(p))$ not in $\mathfrak{R}$ and $P(\sigma(p))$ not in $\mathfrak{R}$.

The first alternative is excluded whenever $\mathfrak{P}$ is a consistent class (at most one of the two expressions A and $\rightarrow A$ belongs to it) ; then neither $P(\sigma(p))$ nor $\rightarrow P(\sigma(p))$ is provable : this establishes the *incompleteness* of any system to which the foregoing method can be applied. The method gets its flexibility from a suitable choice of $\mathfrak{P}$.

A slight variant, which is often useful, holds under different conditions, namely when (the provability of) $P(n)$ can be *interpreted* by reference to the property $\mathfrak{P}$ as follows :

if N is in $\mathfrak{P}$ then $P(n)$ is in $\mathfrak{R}$;
if $\rightarrow N$ is in $\mathfrak{P}$ then $\rightarrow P(n)$ is in $\mathfrak{R}$.

Thus, taking $\rightarrow P(\sigma(p))$ for N , we have :

if $\rightarrow P(\sigma(p))$ is in $\mathfrak{P}$ then $P(\sigma(p))$ is in $\mathfrak{R}$;
if $\rightarrow \rightarrow P(\sigma(p))$ is in $\mathfrak{P}$ then $\rightarrow P(\sigma(p))$ is in $\mathfrak{R}$.

If $\mathfrak{R} \subseteq \mathfrak{P}$ and either $P(\sigma(p))$ or $\rightarrow P(\sigma(p))$ is in $\mathfrak{R}$ (and therefore in $\mathfrak{P}$) then $\mathfrak{R}$ is inconsistent.

Gödel's second step, when applied to (S) , comes to this : the formula

$$\Delta \rightarrow \rightarrow P(\sigma(p))$$

can be proved in (S) itself where Δ is a counterpart in (S) of the statement ' (S) is consistent', and the property $\mathfrak{P}$ is simply that of being provable in (S) . (Actually, $\Delta \leftrightarrow \rightarrow P(\sigma(p))$ can be proved, but this is not needed.) Thus, under the conditions stated, there can be no proof of Δ in (S) .

These results have been described as devastating. This, in my opinion, gives a wrong picture of the matter. It is true that all existing sketches of consistency proofs for number theory could be shown to be sterile ; but then—I think it is correct to say this—many people were suspicious of them anyway. Before Gödel's work appeared there was simply no

effective check on a suggested method of attack, there was nothing to make it either plausible or implausible. After Gödel one had a test which threw out many suggestions for a consistency proof; this very fact made those which passed the test worth more attention.

It is as well to note that this test is only negative. Gödel's work showed that a method which could be formalized in the system considered was pointless (except for producing an inconsistency); but the problem of establishing that a method cannot be formalized, that is, that by means of it formulae could be proved which are not provable in the system, is at least as difficult as a consistency proof, since in an inconsistent system every sentence can be proved. Thus, on the positive side the test consisted in examining whether the method *looked* like going beyond what could be formalized in the system.

Non-mathematical readers may think that introducing new methods of proof presents particularly great difficulties. There is no doubt that Tarski, Gentzen, Ackermann, Schütte and others who have used certain forms of transfinite induction, which could not be formalized in (S) , did overcome real difficulties, but an interesting proof of Δ of a relatively simple kind was, after all, given about six years after Gödel's paper. On the other hand famous problems which were eventually decided by methods of (S) were studied vigorously for forty or fifty years before they were solved and then one had to start again to get a really elementary treatment. (For example, Riemann's conjecture of the prime number theorem in 1851, its proof by Hadamard and de la Vallée Poussin in 1896, and the elementary proofs by Selberg and Erdős in 1950.)

5. Now we come to the exposition given in the book under review. A substantial part of the work consists of setting up an expression $(Ev_k)\Phi(a_1, v_k)$ of (S) such that $(Ev_k)\Phi(n, v_k)$ can be proved in (S) if and only if the expression with number n can be proved in (S) : it is in this sense that one speaks of 'the class of numbers of (S) -provable formulae being definable in (S) '. The expression actually has the property required if (S) is ω -consistent. Ω -consistency is not investigated in the book, at any rate not in as thorough a manner as by Hilbert and his pupils.

The representation in (S) of functions defined by (simultaneous) primitive recursions, which is needed in this work, is not a trivial matter. It is done rather elegantly on pp. 14-23, and 32-37.

A second step, the definition of the substitution function, yields an expression σ such that $(Ev_k)\Phi(\sigma(\mathbf{m}), v_k)$ can be proved if and only if $M(\mathbf{m})$ can be proved, where $M(a_1)$ is the expression with number m . By applying the diagonal method one shows that $\neg (Ev_k)\Phi(\sigma(\mathbf{p}), v_k)$ is not (S) -provable, where p is the number of the expression $\neg (Ev_k)\Phi(\sigma(a_1), v_k)$. On further examination—if the system is ω -consistent—it is found that $(Ev_k)\Phi(\sigma(\mathbf{p}), v_k)$ is not (S) -provable either.

To get Rosser's well known result¹ one defines the class $\mathfrak{R}$ of provable expressions N by the condition $\mathfrak{P}$ that N can be proved by a proof with number p , such that $\rightarrow N$ is not proved by proofs with numbers $\leq p$. The application of the diagonal method to (the definition of) this class $\mathfrak{P}$ leads to undecidable expressions in consistent extensions of (S) , even if they are not ω -consistent. Recalling paragraph 4, one could put it this way: the expression in (S) which Rosser introduces can actually be interpreted by reference to the class $\mathfrak{P}$ if (S) is simply consistent.

Note that when one considers the class of provable expressions, one lumps together (in its complement) disprovable and undecidable ones; an expression N is called disprovable if $\rightarrow N$ is provable, and, of course, undecidable if neither N nor $\rightarrow N$ is provable.

6. The crux of all this is the existence of expressions in (S) which define, in the sense explained at the beginning of paragraph 5, the class of provable expressions, etc. The author considers the *definability of classes* in a general way.

On p. 74, a property of integers R is said to be $\mathfrak{R}$ -definable by the expression Φ of $\mathfrak{R}$ if, for each integer $1, 2, \dots$, the following two conditions are satisfied:

If $R(n)$ then the expression $\Phi(n)$ is $\mathfrak{R}$ -provable.

If non $R(n)$ then $\rightarrow \Phi(n)$ is $\mathfrak{R}$ -provable.

A few obvious points: if R is decidable for each integer $1, 2, \dots$ and $\Phi(n)$ is $\mathfrak{R}$ -undecidable for some, then R is not $\mathfrak{R}$ -defined by Φ . If $R(n)$ is undecided the two conditions above impose no restriction on $\Phi(n)$. Now if one has to prove $\mathfrak{R}$ -indefinability² (and this is the tricky part) one has to show that for each Φ of $\mathfrak{R}$ there is an n such that for this n the above conditions are not satisfied. And for this n , one would suppose, $R(n)$ is decided. But this is certainly not so with the cases in the book. However, the arguments given in the book enable one to reformulate the results as follows: first, without much trouble one can specify a system $\mathfrak{R}_1$, in which the definition of the property R can be formalized (by the expression $R(a_1)$); next, the arguments suggest a system $\mathfrak{R}_0$ which includes both $\mathfrak{R}$ and $\mathfrak{R}_1$ and, further, has the property that for each Φ of $\mathfrak{R}$ there is an n such that

$$R(n) \longleftrightarrow \rightarrow \Phi(n)$$

is $\mathfrak{R}_0$ -provable. In $\mathfrak{R}_0$, $R(n)$, and hence $\Phi(n)$, may quite well be undecidable. The author dismisses the cases considered above, where $R(n)$ is undecidable, by references to 'the intuitive arithmetic' where $R(n)$ is

¹ J. B. Rosser, 'Extensions of some theorems of Gödel and Church', *Journal of Symbolic Logic*, 1936, 1

² We use 'indefinable' throughout, though the author uses the word 'undefinable'.

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either true or false. This intuitive arithmetic, according to p. 26, is 'known to everybody from . . . school'. Taken literally these references are very confusing: at school $R(n)$ is not only either true or false, but either provable or disprovable; and the quantifiers are quite different from those of (S) , for example existential quantifiers can always be restricted to a limited range. It is, of course, true that the naïve way of speaking about intuitive arithmetic suggests Tarski's very interesting formal development which is described below: but it is only by way of such a development that useful results are obtained and the false extrapolations from those parts of arithmetic which we meet at school are excluded. It is perhaps surprising that the author does not introduce the classes $\mathfrak{R}_1$, and $\mathfrak{R}_0$ explicitly, since this was done by Tarski¹ in work with which the author was associated.

Some further comments on $\mathfrak{R}$ -definability. It is useful to distinguish between a *strong* $\mathfrak{R}$ -definability, when

$$(Av_k)(R(v_k) \longleftrightarrow \Phi(v_k))$$

is $\mathfrak{R}_0$ -provable, and a *weak* $\mathfrak{R}$ -definability when for any n

$$R(n) \longleftrightarrow \Phi(n)$$

is $\mathfrak{R}_0$ -provable. An application of this distinction is provided by *general recursive predicates*: any such predicate is (S) -definable in the weak sense, but only ordinal recursive predicates of order less than the first ϵ -number are (S) -definable in the strong sense.

Several interesting results on definability, due to Kleene, and to Mostowski himself, are not mentioned in the book. It seems worth while to give a formalized version for the simplest case because it makes the method of indefinability proofs clear. It is this:

There is a primitive recursive relation $\overline{\Xi}(v_1, v_2, a_1)$ such that

$$(Ev_1)(Av_2)\overline{\Xi}(v_1, v_2, a_1)$$

is not definable by an expression $(Av_1)\Psi(v_1, a_1)$ where Ψ is also primitive recursive.

We use the following (known) facts about (S) . There is an expression $\phi(a_1)$ of (S) which represents a primitive recursive function $f(n)$ such that the values of $f(1), f(2), \dots$ are the numbers of expressions $\Psi_1(v_1, a_1), \dots$ of (S) which represent all primitive recursive relations. (Each such relation can be represented by some expression with number $f(n)$, but, of course, not all expressions of (S) which represent such relations have numbers $f(n)$.)

There is an expression $S(a, b, \phi(c))$ which represents the function whose value is the number of the expression $\Psi_c(a, b)$.

¹ A. Tarski, 'Abstract', *Journal of Symbolic Logic*, 1949, 14, 75

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Then, as shown by Bernays,¹ the expression

$$\Psi(\nu_1, a_1) \rightarrow (E\nu_2)\Phi(S(\nu_1, a_1, \phi(\mathbf{n})), \nu_2)$$

can be proved in (S) where $\Psi(\nu_1, a_1)$ is the expression with number $f(\mathbf{n})$.

Similarly, if the expression $\rightarrow\Psi(\nu_1, a_1)$ has the number $f(\mathbf{m})$

$$\rightarrow\Psi(\nu_1, a_1) \rightarrow (E\nu_2)\Phi(S(\nu_1, a_1, \phi(\mathbf{m})), \nu_2)$$

can be proved in (S). Hence, if the expression Δ of p. 104 is the arithmetical counterpart of the consistency of (S)

$$\Delta \rightarrow . \Psi(\nu_1, a_1) \longleftrightarrow (E\nu_2)\Phi(S(\nu_1, a_1, \phi(\mathbf{n})), \nu_2)$$

and

$$\Delta \rightarrow . (A\nu_1)\Psi(\nu_1, a_1) \longleftrightarrow (A\nu_1)(E\nu_2)\Phi(S(\nu_1, a_1, \phi(\mathbf{n})), \nu_2)$$

are both (S)-provable.

The expression

$$(E\nu_1)(A\nu_2) \rightarrow \Phi(S(\nu_1, a_1, \phi(a_1)), \nu_2)$$

is not definable by an expression

$$(A\nu_1)\Psi(\nu_1, a_1)$$

since, for each Ψ , with number n ,

$$\Delta \rightarrow . (A\nu_1)\Psi(\nu_1, \mathbf{n}) \longleftrightarrow \rightarrow(E\nu_1)(A\nu_2) \rightarrow \Phi(S(\nu_1, \mathbf{n}, \phi(\mathbf{n})), \nu_2).$$

Write $\equiv (\nu_1, \nu_2, a_1)$ for $\rightarrow\Phi(S(\nu_1, a_1, \phi(a_1)), \nu_2)$.

The system $\mathfrak{R}_0$ in which the indefinability is proved is simply (S) with Δ as an additional axiom.

My own impression is that the notion of $\mathfrak{R}$ -definability is very interesting, *i.a.* it allows the important generalisations on pp. 97-99 to be formulated crisply, but the pattern of the incompleteness proofs themselves is made just as clear by the elementary formulation of the diagonal argument in paragraph 4 above.

Note.—The conditions imposed in paragraph 4 on $P(\mathbf{n})$ are variants of $\mathfrak{R}$ -definability; thus the first set reads:

- $$\left. \begin{array}{l} \text{(i) if } N \text{ in } \mathfrak{P} \text{ then } P(\mathbf{n}) \text{ in } \mathfrak{R}, \\ \text{(ii) if } P(\mathbf{n}) \text{ in } \mathfrak{R} \text{ then } N \text{ in } \mathfrak{P}; \end{array} \right\} (G)$$

and the second set reads

- $$\left. \begin{array}{l} \text{(i) if } N \text{ in } \mathfrak{P} \text{ then } P(\mathbf{n}) \text{ in } \mathfrak{R}, \\ \text{(ii) if } \rightarrow N \text{ in } \mathfrak{P} \text{ then } \rightarrow P(\mathbf{n}) \text{ in } \mathfrak{R}. \end{array} \right\} (R)$$

But in the applications described in paragraph 5 we do not meet the ambiguities in the interpretation of logical connectives which occur in the general case. For the property $\mathfrak{P}$ is simply the property of being (S)-provable, and the implications (G) and (R) are established by describing effective methods of converting (α) any given (S)-proof of N into an (S)-proof of $P(\mathbf{n})$, for (R)(i) and G(i), (β) one of $P(\mathbf{n})$ into one of N , for G(ii), and (γ) one of $\rightarrow N$ into one of $\rightarrow P(\mathbf{n})$, for R(ii).—There can be no analogue in the general case because in informal arithmetic the methods of

¹ Hilbert Bernays, *Grundlagen der Mathematik*, vol. II, p. 312

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proof are not delimited, and therefore it does not make sense to ask for a method of converting *any* informal proof of $R(n)$ into a $\mathfrak{R}$ -proof of $\Phi(n)$. The particular case when $R(n)$ is systematically decidable is only an apparent exception: for *systematic* decidability means no less than decidability by methods which are *restricted in advance*, in other words they are no longer informal.

7. A particularly simple application of the diagonal argument shows that in (S) there is no expression $\Phi(a_1)$ such that

$$\Phi(n) \longleftrightarrow N$$

can be proved in (S) for each sentence N (with number n). Thus the problem arises, to find an extension (S_1) of (S) with an expression $\Phi_1(a_1)$ such that

$$\Phi_1(n) \longleftrightarrow N$$

can be proved in (S_1) for each sentence N of (S) .

Some quite trivial extensions (S_1) of (S) in which there are such expressions $\Phi_1(a_1)$ can be written down. But Tarski gave a rather interesting system (S_1) , a formalization of set theory, in which he constructs an expression $\Phi_1(a_1)$ with the required property. He calls such an expression a *truth-definition* for (S) .

Actually, if m is the number of the sentence $\rightarrow^* N$

$$\Phi_1(n) \vee \Phi_1(m) \text{ and } \Phi_1(n) \rightarrow \rightarrow \Phi_1(m)$$

can also be proved in (S_1) . The experienced reader will not misinterpret these results: e.g. the former does not mean that either $\Phi_1(n)$ or $\Phi_1(m)$ can be proved in (S_1) , no more than the provability of $N \vee \rightarrow N$ in (S) means that either the sentence N or the sentence $\rightarrow N$ is provable in (S) .

Two interesting uses of the expression $\Phi_1(a_1)$ should be noted: (i) It is an example, perhaps the simplest of its kind, of an expression of (S_1) which can be proved in (S_1) to be indefinable by an expression of (S) , (ii) the various $\mathfrak{R}$ -indefinability results can be proved in (S_1) via the expression $\Phi_1(a_1)$.

I do not think that the interest of the expression $\Phi_1(a_1)$ suffers from being related to the system (S_1) instead of to 'intuitive arithmetic'.

Incidentally, the author says on p. 60 that the notion of an arbitrary set cannot be avoided in a truth-definition for (S) . This is false.¹ What is true is that the definition in (S_1) is more interesting than the alternative of merely adding the defining schema of Bernays and the axiom Δ to the system (S) . It should be noted that relatively little is known about truth-definitions for (S) , for example whether a truth-definition could be given in an extension of (S) by some principle of ordinal induction up to an ϵ -number in the second number class (naturally greater than the first ϵ -number).

¹ See, for example, Hilbert Bernays, *Grundlagen der Mathematik*, vol. II, p. 334.

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8. In one respect the treatment is one-sided. All the undecidability results are treated as proofs of the inadequacy of the systems considered—a constant plaint throughout. But equally one may point out that they show this: any proof in a formalized system of arithmetic gives information not only about the integers, but about concepts which are essentially different from the integers (a kind of unexpected or unintended efficiency of the system). These concepts provide the so-called *non-standard models* of a formalized system, where the individuals are, say, the integers 1, 2, . . . and in addition $a_\omega, a_{\omega+1}, \dots$ (there is no suggestion that they must be well ordered). Now, consider one of the undecidable expressions

$$(Av_k) \rightarrow \Phi(\sigma(\mathbf{p}), v_k).$$

It might well be that $\rightarrow \Phi(\sigma(\mathbf{p}), 1), \rightarrow \Phi(\sigma(\mathbf{p}), 2), \dots$ all represent true relations (in the model), but $\rightarrow \Phi(\sigma(\mathbf{p}), a_\omega)$ does not. In fact, one can always define such a model. But if the system is to be ω -consistent, $(Ev_k)\Phi(\sigma(\mathbf{p}), v_k)$ can only be proved if some $\Phi(\sigma(\mathbf{p}), \mathbf{m})$ represents a true relation, for example $\Phi(\sigma(\mathbf{p}), a_\omega)$ is not enough.

It is more interesting to consider Rosser's case from this point of view.

$$(Av_1)(Ev_2)(\Phi(\sigma(\mathbf{p}), v_1) \rightarrow \cdot v_2 \leq v_1 \ \& \ \Phi(N\sigma(\mathbf{p}), v_2)).$$

$N(\sigma(\mathbf{p}))$ represents the number of the negation of the formula with number $\sigma(\mathbf{p})$.

Here, also in non standard models $\rightarrow \Phi(\sigma(\mathbf{p}), \mathbf{m}), \rightarrow \Phi(\sigma(\mathbf{p}), \mathbf{m})$. But if, e.g. a_ω is the only individual a for which $\Phi(\sigma(\mathbf{p}), a)$ represents a true relation, Rosser's formula is false in the non standard model; if $\Phi(N\sigma(\mathbf{p}), a_\omega)$ is true, and $\Phi(\sigma(\mathbf{p}), a_{\omega+1})$ is true and $a_\omega < a_{\omega+1}$, the formula is true in the non standard model (true = provable for suitably defined $a_\omega, \dots$).

Also the Skolem model which belongs to the same circle of ideas is more intelligible when considered as a non-standard model.

9. Note in passing that the work of Gödel¹ has also been extended in a different direction, mainly by Bernays.² These developments are hardly touched. They are required for a respectable proof of theorem 1 in the appendix. The author points out that his proof is imperfect, but one can hardly agree that 'the enterprise' of filling in the gaps 'is not difficult in itself': at any rate, as a sheer piece of technique, the work of Bernays referred to is as difficult as anything in the book under review.

The omission is not serious since the proofs are available in full detail.

10. The book presents, as far as I know, the first serious attempt to exploit the basic ideas of Gödel's famous paper.¹ The attempt is, in my opinion, completely successful. The author not only brings together theorems analogous to those of Gödel, which he and others have established for various classes of formal systems of arithmetic, but he discusses the sets of conditions, which are imposed on the systems, in an unusually

¹ See above, p. 366, n. 1

² See above, p. 371, n. 1

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thorough way. (i) He indicates, usually by clear hints, that they are necessary, that is, that there are systems which satisfy all but one of the conditions imposed, but fail to satisfy the conclusion, and (ii) on the very informative pp. 97-102, he shows that the sets of conditions are *incomparable*, that is, taking two sets of conditions at a time, he exhibits formal systems which satisfy one set, but not the other, and *v. v.*

G. KREISEL

Aristotle's Prior and Posterior Analytics. A revised text with introduction and commentary by W. D. Ross (Clarendon Press, Oxford, 1949. Pp. x + 690. 42s.)

THIS book has been reviewed from a technical point of view in other journals; here attention will be drawn to some of the reasons why these logical writings of Aristotle may still be studied with profit by students of the history and philosophy of science. Some of his contributions to the problem of explanation, which was for him, as for most Greeks, the central problem in the philosophy of science, will suffice by way of illustration.

Aristotle distinguished between what he called knowledge of 'the fact' ($\tau\omicron\delta\ \delta\tau\iota$) and knowledge of 'the reason for the fact' ($\tau\omicron\delta\ \delta\iota\omicron\tau\iota$). The latter was scientific knowledge provided by prior principles from which 'the fact' could be deduced. The deduction, or 'demonstration', explained 'the fact', just as the demonstration of conclusions from premisses in a geometrical theorem explained the conclusions. The whole process of searching for scientific explanations of physical phenomena Aristotle regarded as going from the observed to the unobserved. As he said: 'even if it were possible to perceive that a triangle has its angles equal to two right angles, we should still be looking for a demonstration . . . ; for perception must be of a particular, whereas scientific knowledge involves the recognition of the commensurate universal. So if we were on the moon, and saw the earth shutting out the sun's light, we should not know the cause of the eclipse; we should perceive the present fact of the eclipse, but not the reason for the fact at all, since the act of perception is not of the commensurate universal' (*An. Post.*, I, 31).

Aristotle went on to say that we might be led to a demonstration 'by watching the frequent recurrence of this event'. His treatment of the psychological act by which the $\nu\omicron\upsilon\varsigma$ grasped the universal after frequent perceptions of singulars, and of the logical problem of induction arising from the impossibility of a complete enumeration of singulars, are some of the most discussed aspects of his philosophy of science. A practical contribution of great importance which he made to the procedures for searching for the premisses for scientific explanations was his method of

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forming a definition of a species (*An. Post.*, II, 13). He enumerated a collection of attributes belonging to the species which were individually of wider extent than the species, but were collectively coextensive with it. The definition so reached showed the empirical connection between these attributes (or events) in the species.

Another of Aristotle's valuable contributions was his conception that some physical sciences were 'subordinate' to corresponding branches of mathematics. For example, astronomy was subordinate to geometry, in the sense that the movements of the heavenly bodies could be deduced from the geometry of the circle. Optics was also subordinate to geometry, and music subordinate to arithmetic.

The greatest disadvantage from taking Aristotle as a guide to the logic of science—a role he was destined to play for many centuries—derived perhaps from his advocacy of what has been called an 'essentialist' conception of theories. 'The nature of the thing and the reason for the fact are identical', he said (*An. Post.*, II, 2). So, in trying to find an explanation of an event, he looked for a substance into whose definition the event entered as an attribute. One obvious defect of this way of talking about events is that it did not allow for certain kinds of comparison. For example, the predicates of the sentences 'A is larger than B' or 'A is moving faster than B' do not designate attributes of the subject A in the same way as does the sentence 'A is blue'. In Aristotle's language a science like dynamics would be impossible, and, in fact, one of the main difficulties encountered in the invention of modern dynamics, between the fourteenth and the seventeenth centuries, was that scientists steeped in Aristotle's thought had to invent a new scientific language to deal with problems involving rates of change.

These sketchy indications are intended to do no more than arouse interest in these earliest systematic treatises on the logic of science. This will no doubt become the standard edition of the Greek text. The introduction and notes are learned and illuminating, as we might expect from our leading Aristotelian scholar.

A. C. CROMBIE

Measurements of Mind and Matter, G. W. Scott Blair (Dennis Dobson Ltd., London, 1950. Pp. 115. 9s. 6d.)

Dr SCOTT BLAIR writes in his preface that his book sets forth 'some of his ideas' on problems in the theories of measurement and dimensions that he has met in his own special field of applied rheology, its contacts with psychology, and elsewhere. The author has not only set himself the task of writing on problems whose discussion inevitably takes him outside his own

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field—he dedicates his book to ‘all those who are not afraid to cross frontiers’—but of doing so in a short space and in a way understandable to the non-scientist. Presumably for the last reason he has chosen not to introduce his subject matter to the reader by way of the problems which were his own starting point. The result is unfortunate.

As it is, the book lacks a clearly defined purpose and development and tends to consist of an assembly of observations and suggestions rather than a connected discussion. The introductory chapter on ‘What this book is about’ is examined in vain for more than a reiteration in the final paragraph that the book is to be about measurement. After the further interruption of an ‘Interlude’ on ‘What is meant by dimensions’ for the uninitiated, which like other similar explanations might better have been made an appendix, the book proceeds by way of chapters on the principles of measurement, the physical theory of dimensions and physical properties. The substitution of a wealth of quotation, here as elsewhere, leads to obscurity both as to intention and outcome. These chapters are presumably to pave the way for the author’s own ideas on ‘dimensional intermediacy’ and ‘quasi-properties’ which appear in the three concluding chapters. Here extensive use is made of psychological analogies; there can be no quarrel with this if it helps the author to think about physics, but the process is of doubtful reversibility.

Sandwiched between these two sections of the book is the chapter on ‘Mental measurements and psychological dimensions’. It is but little related to what follows and less to what has gone before. The author is aware of the dangers of brevity and lack of first-hand information in this kind of excursion; he can not on that account escape the criticism that in this chapter he misleads by having attempted to say too much about a selection of examples in too little space. The criticism applies particularly, for example, to his attempt to describe how and why psychologists use measures of intelligence and it is again to be doubted whether the compressed account of factor analysis, a controversial subject in psychology, gives any indication other than a confused one of why it has been used by some psychologists. Not all do; many more are concerned with the investigation of psychological processes than are convinced of the necessity of seeking psychological dimensions. The author should also observe that many psychologists do not suppose themselves to make ‘mental measurements’. Nothing is said in general about the problems of observation and quantification in psychology, matters in which psychology is much more nearly related to other biological sciences than it is to physics.

The book is at its happiest with its elementary explanations of physical concepts, but the non-physical-scientist will conclude that non-Euclidean geometry remains as much of a mystery as ever when, at the end of an otherwise illuminating appendix on Gaussian co-ordinates, he reads that

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the curves in an accompanying diagram have been drawn 'almost as if they were radii of two circles'.

The author is sincere in his belief that scientific frontiers should be crossed ; but they should perhaps be bridged and his book, which is not a careful examination of the relations between problems of measurement in physics and psychology, for all his good intentions, presents a confused picture of psychology to physicists, of physics to psychologists and of both to the non-scientist.

A. SUMMERFIELD

Von Ursprung und Grenzen der Geisteswissenschaften und Naturwissenschaften,
Ernesto Grassi und Thure von Uexkühl (Verlag A. Francke A.G.,
Bern, 1950. Pp. 252. Fr. (Swiss) 12.60)

THIS book consists of two essays, the first by Professor Grassi and the second by Dr von Uexkühl. There are altogether 252 pages in the book, and it is only when we have arrived at page 198 that we are really in a position to understand what it is that connects the two essays and justifies their appearance under one cover as parts of a single book.

The unifying idea is that both humane studies (Grassi) and natural science (von Uexkühl) result from the unique position which man holds in the animal kingdom. Man, so these authors maintain, has lost the primal innocence and security of the beasts and, no longer safely conveyed by instincts, which if not infallible are for the lower animals unquestioned, must fashion explicit rules of conduct both in the ethical and in the practical sphere.

Professor Grassi sets himself to attack two problems : What place has natural science in education and what place have the humanities ? What precisely is the nature of the humanities considered as instruments and ends of education ?

The first of these questions receives an exact answer : the natural sciences are merely specialist studies (*Fachwissenschaften*) and have no place in general or liberal education. The second question receives a highly suggestive treatment, but no exact answer.

It does not appear to me that Professor Grassi's treatment of the natural sciences is particularly novel or interesting. He seems to regard them as mere technologies, as subjects studied only for their immediate usefulness. He appears to have no notion of their aesthetic appeal. His essay becomes more interesting when he turns to the consideration of humane studies. Illustrating his thesis with quotations from Vico he draws a seductive picture of the origin of these studies, which he defines as 'the study of what it means to be a man' (p. 120). With Vico he supposes a sudden

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emergence of man from the lower animals ; the cause of man's emergence is an unexplained revulsion (Schrecken) against his animal nature. Under the drive of this revulsion man, as Vico picturesquely says, emerges from 'the bush' and clears himself fields ; not rejecting the forces of nature (including his own instincts), but disciplining them. The process of ordering and disciplining is no mere negation or puritanism, but is positive and constructive. Its centre of reference is the Myth. Just as we are not at liberty to construct capricious systems in natural science, so we must seek a myth which is consonant with reality. It is the search for and promulgation of the realistic myth which constitutes the proper task of humane and liberal studies and education.

Grassi leaves us, however, with very little idea of how we are to find the myth appropriate to our temporal circumstances. We are to seek the Good, in Aristotle's sense of the word ; but it does not appear, indeed it would seem to be entirely against the spirit of myth-making, that we should simply conduct our lives according to a scheme based directly upon Aristotelian teaching. There seems, also, to be some contradiction in the conscious adoption of a myth. Does not the force of a myth reside precisely in the belief that it is *not* a myth ?

Dr von Uexkühl's principal aim appears to be to assert the primacy of the biological as against the physical sciences. His thesis is that the entities which physicists discuss are merely a set of carefully selected abstractions from the totality of human experience and, further, that all human experience and observation necessarily suffers from the limitation of *being* human. The author contrives to induce in the reader the feeling that he is being treated to something very novel.

In examining the parts of the essay one finds much that it is difficult either to accept or to pass over in silence. Dr von Uexkühl tries to persuade us that some profound revolution takes place in physics when it is realised that the standpoint of the observer must be considered in the evaluation of his measurements, and seems to give Professor Einstein credit for the discovery of this truism. 'The reality of man, who does the measuring, has thereby become formidably enlarged. He no longer stands rooted in the middle of the landscape, but constantly changes his standpoint, to get each time a new view of things' (p. 133). But surely this very metaphor, which seems to be taken from triangulation, should make us cautious in asserting that a man needs a knowledge of modern physics to appreciate the importance of the observer's point of view. In general Dr von Uexkühl has a curious blindness for trigonometry. He asserts that the results of physics are 'purely quantitative' (e.g. p. 134) and really does not seem to appreciate that the three dimensions of our physical world are not quantitatively but qualitatively distinct from one another. This blindness to *direction* appears again in the very odd passage in which he tries to

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tell us how the dimensions of physics are sorted out from the sum of our raw experiences. Neither his account of man's construction of physical space, nor his reference to the bat's auditory space takes any account of the implicit use of direction-finding in eye-muscle sense or in retinal or aural comparison. His account of the origins of human conceptions of number and space seems to me so perverse that I suspect I must have misread it. It looks to me as though he wants us to imagine primitive man constructing the number system and space *by adding smallest perceptible differences*.

There is much else in both essays that one could criticise in detail without pedantry. In particular the printing of the quotations in Greek, Latin and even French is really terrible. If it was impossible to get these adequately printed, they should have been omitted. This criticism refers to Professor Grassi's essay; Dr von Uexkühl has not, I believe, a single reference to any non-German author. This seems to me a defect of some consequence. Especially in an essay devoted entirely to theoretical biology, a reference to the work of Professor Woodger would have been, to say the least, graceful. A little of Professor Woodger's astringent spirit would have done no harm. In speculations about the construction of space it is really unreasonable to omit all reference to Professor Jean Piaget's brilliant and instructive work on children. And where, among the German authorities, is Frege?

The general tendency of this book, in so far as it aims at the encouragement of a broad humanistic outlook, is admirable; but the authors, I think, promise more than they perform. I do not think that anyone not already much in sympathy with the general positions of the work would find its argumentation very convincing.

J. S. WILKIE

The Conditions of Knowing, Angus Sinclair (Routledge & Kegan Paul Ltd., London, 1951. Pp. 260. 21s.)

THIS book is the expression of an unfashionable belief in the importance of epistemology. Its main thesis is given in the opening paragraph: 'It appears to me that underlying the various epistemological views or theories or attitudes characteristic of the European-American culture is some fundamental epistemological attitude or assumption, or complex of attitudes or assumptions, which is or are inadequate and misleading. The epistemological writings of recent times can be regarded as a record of the progressively clearer recognition that this is so, and of the progressively clearer indication of some better alternative.' The main difficulty in supporting this thesis arises from the impossibility of formulating explicitly this inadequate theory, or the preferred alternative to it. We cannot detach

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ourselves from our epistemological attitudes and view them from outside, as it were. 'The only workable method is not to discuss the surmised supreme presupposition or attitude itself but the less general theories or attitudes on what we can call the level next below, in the hope that this will result in amending the supreme attitude by its repercussive effect.' Accordingly, the bulk of the book consists in discussions of particular epistemological topics, designed to show that the inadequacy of the traditional treatment is a *general* inadequacy, symptomatic of some general epistemological fallacy. There is not, for example, one problem of Meaning, and another problem of Truth, and another problem of Explanation, for these sub-problems are so many instances of the same fundamental issue, which recurs in different contexts. This brings me to my first criticism of Mr Sinclair's thesis. On his own admission, he does not succeed in exhibiting clearly the surmised connection between the many problems he discusses. He admits that he has found it possible only to make limited generalisations in certain restricted fields. In view of this, I cannot understand his confidence that there is one supreme presupposition which determines the treatment of topics which appear to be relatively independent of one another. Mr Sinclair seems to have given his unifying attitudes too free a rein. The clearest example of this occurs in a chapter headed 'The Criterion', in which five ways of evaluating theories are discussed. It is by no means evident that these five ways 'really' constitute one way, and Mr Sinclair candidly admits failure in his attempt to work out a generalised account such that these five criteria could be regarded as special cases of the same general principle. But he maintains nevertheless that this 'ought to be possible'. It would seem better to conclude that these five ways are complementary to one another, rather than exaggerate their unity.

My other main doubt concerns the nature of the alternative theory which is advocated. Roughly, this seems to consist in re-interpreting the traditional distinction between a subject who knows and an independent object which is known in terms of the different experiences and attitudes of individual men. Thus, the traditional theories of truth are criticised on the grounds that they presuppose the existence of independent facts and propositions. The epistemological situations which these theories were meant to account for are said to be better explained in terms of the different experiences induced by true and false statements, as these are traditionally called. What then is the difference between the 'fictitious' experiences or attitudes of the novelist and the 'truthful' attitudes of the historian? Mr Sinclair seems to give more than one answer to this. First, he suggests that the attitudes of the historian can account for the relevant data more adequately than those of the novelist. But later he writes: 'Where we have a body of statements that are coherent among themselves, we have what we call fiction, and when these are coherent with all of our experience,

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then we have what we call truth'. On the first formulation, truth is defined in terms of a relationship between personal attitudes and 'data', on the second, in terms of the coherence of experience as a whole. This inconsistency may be only apparent, on Mr Sinclair's own premises. But I cannot see that he has here offered any basically-different alternative to the traditional theories which, as he claims, rest on a misleading assumption. The other discussions in this book do not make this alternative any clearer to me.

R. J. SPILSBURY

A Study in Memory—A Philosophical Essay, E. J. Furlong (Thomas Nelson & Sons Ltd., Edinburgh, 1951. Pp. 109. 12s. 6d.)

IN this essay Professor Furlong attempts a rebuttal of the sceptical arguments against the trustworthiness of memory. After rejecting the views that in recollection a man is directly acquainted with the remembered past, and that the problem of justification is a pseudo-problem, he considers whether memory-beliefs can be verified by experience. The common-sense view is that empirical confirmation of the reliability of memory-reports is possible, and Professor Furlong accepts this view, arguing that the validating tests are the same as those used to challenge the reliability of any witness or informant. He admits, however, that such tests cannot dispense with some 'appeal to memory'. 'The only way in which we could decide on the reliability of memory itself is by remembering that its past information has been accurate. . . . We cannot validate memory without assuming memory.' But the fact that all the conventional tests of reliability presuppose memory in this sense seems to Professor Furlong to take the sting out of this admission. I doubt whether any except faint-hearted sceptics would agree with this.

What is insufficiently emphasised is that the predicament, if it is felt as such, is a general one and not peculiar to memory. Perceptual reports are tested perceptually, mathematical conclusions mathematically, historical judgments historically, aesthetic criticism aesthetically. . . . The purpose of these checks is to minimise the risk of bias and error in particular cases, by comparing them with other judgments in the same category. The demand that judgments in one category should be confirmed solely by reference to judgments in other categories seems senseless.

Professor Furlong ends his essay with some interesting remarks on the psychology of remembering, his main point being that what is reproduced is the whole 'state of mind' of the person in his previous situation, and not a mere flicker of images. If he had developed this point, he might perhaps have been led to reformulate his problem in terms of dispositions, and to

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ask, for instance, what it means to say that a dispositional state can persist unchanged, or that 'the same' disposition can recur on different occasions. But this, no doubt, would have meant enlarging the scope of his essay.

R. J. SPILSBURY

Space-Time-Matter, Hermann Weyl, translated from the German by HENRY L. BROSE (Dover Publications, Inc., New York, 1951. Pp. xviii + 330. \$3.95.)

THIS is the first American printing of the English edition (1922) which was translated from the fourth German edition. Professor Weyl's book was first published in 1918, and it has since become known to generations of students as a most complete account of relativity theory. The emphasis is on *affine geometry* by means of which a unification of gravitation and electromagnetism is sought. The frequent philosophical asides occasioned by this attempt have always been of great interest to the critical physicist. Obviously, a detailed review of this book would be out of place.

However, I should like to draw attention to the comments which the author makes on his work in the new preface to the American edition. In the last two chapters of the book, the principle of *gauge invariance* is introduced in order to achieve a unified field theory. Professor Weyl states flatly that 'this attempt has failed'.

There are many reasons for this, not the least that the wave field of an elementary particle (whose existence was then not suspected) has to be added to any unified theory. The problem of interaction of particle and field is, today, far from being solved. The author believes that a solution 'may well require a deep modification of the foundations of quantum mechanics'.

Most physicists, I suppose, now no longer expect that a unification through the concept of field is possible, that is, that the ideas of continuity and of discreteness can be reconciled in this way. The essentially statistical character of quantum mechanics makes it more likely that a *fundamental length*, and possibly a *fundamental time interval*, will have to be introduced; and such concepts have no application in a theory which is based on continuity. But whatever the future may bring, Professor Weyl's book will remain a classic of physics.

E. H. HUTTEN

The Scientific Adventure, Herbert Dingle (Sir Isaac Pitman and Sons, Ltd., London, 1952. Pp. x + 372. 30s.)

READERS of this *Journal* require no introduction to Herbert Dingle. It was he who contributed the first article published in volume one, and

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whose interest has been central to its growth. His influence as professor of the History and Philosophy of Science at London, and the marks of his thought as centre of a most active research group at University College, are to be found wherever British scientists look up from their preoccupation with doing an experiment to ask, 'What am I really doing?'. The present book consists of reprints of some twenty lectures written in the last fifteen years. One half are labelled Historical and the other half Philosophical.

Let it be said at once that this book is a joy to read. In older times—the days of Newton or Maxwell or Stokes—there was a dignity and a spaciousness about the writings of British scientists which made some of them into works of art. Such literary ability is rarely found in the rush of today. But Dingle is in the old tradition; he writes with an ease and charm that made at least one other scientist envious. And he brings wit and delicacy of touch even when he shows his characteristic fighting spirit. There are very few people who can depict history so vividly and penetrate so swiftly to its main factors. Time after time, as I read the first half of these essays, I said to myself: 'That's absolutely right, but I never thought of it like that'. If it is a healthy exercise to trace the development of one's own discipline, then here, particularly in mechanics and astronomy, is good medicine.

The second group of essays, gathered under the title 'Philosophical', represent a sustained attempt to describe the revolution that is still taking place in science, more particularly in its interpretation and the claims that it makes to represent reality. Dingle starts where Kant started, with the absolute primacy of experience, the only unchallengeable and indubitable truth that we can be aware of. Science represents one technique for rationalising these experiences. 'If one wants to achieve a complete rationalisation of experience, the scientific, though incomplete, is the only one with any chance of success.' Each separate science is a set of models, useful for a time but ultimately out-of-date as experience grows. But none of the models is 'true', unless by truth you happen to mean 'suitable for present purposes and self-consistent'. The models suitable for different purposes may appear to conflict, but that is simply because they *are* suitable for different purposes, and this should not alarm us. Any possible conflict between science and religion falls into such a class, 'phantom problems' in the language of Max Planck. Thus cosmology can be described in scientific terms, in which case nebulae and red shifts are the proper concepts to use; but it is equally proper to describe it in religious terms, where words like splendour, grandeur, majesty, and God find their correct employment. I think Dingle would say that it was the same basic experience that was being interpreted in both cases. On the other hand there are many experiences for which only one type of concept is at present satisfying.

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The situations of ethics, regarded as answers to the question 'How shall I choose between one or the other course of action at this very moment?' do not belong to science at all. But the question: 'Why did I choose this or that?' most certainly does.

One important thing about all this is that it only changes interpretations; it leaves the mathematical relationships expressed in scientific laws quite unaffected. Bohr's principle of complementarity goes by the board (it is only referred to once, and then to show its non-conformity with this operational viewpoint), since when you say that you may only have a degree of knowledge of such variables as the position and momentum of a particle as satisfies the familiar inequality $\Delta p \Delta q \geq h/4\pi$, it amounts to your making an assertion that there really is such a thing as a particle, some objective part of nature which can be studied and measured; and this is still true even if the study is only in probabilities. But if nature has been almost equated with the laws of reason, and if all objective character has been denied to it, then there is no particle to have these spurious properties—of position and momentum. The inequality remains, but serves to describe in an arithmetical manner the results of measurements. Strictly speaking, it has no meaning.

This is all very exciting, and Professor Dingle quite rightly calls his book *The Scientific Adventure*. It is also extremely desirable that this point of view should be stressed, and stressed so convincingly as it is here. Yet I wonder whether it is as comprehensive an account as some of his followers have sometimes claimed. There are at least three points at which I find myself sticking. All three seem to me to urge that Dingle's viewpoint, valuable as it is, does not represent the last word that can be said. (To be fair to the author, he does not claim that it does, so that the following comments are rather in the nature of a tentative feeling-forward for the next step.)

In the first place, experiences, which form the basis of all scientific accounts, do not just 'come' to us. They are sought for. No-one does an experiment 'just for the fun of the thing', but with a definite aim in view. It is surely grossly incomplete to say, 'when I look into the sky, I do not choose what I see; I see what I see'. I am not a kind of magnificent electronic machine which receives units of information from all over the place—like radio weather reports from stations all round the world—and then sorts them, collates them, unifies them, and then, by means of various feedback mechanisms, makes certain conclusions, emits certain units of information for others, and, in short, behaves in certain definite ways. If I am described in terms like these, however refined the description may be, then I believe that something has been missed out. Plotinus' great question 'But who are we?' requires an answer before even Dingle's analysis can be properly appreciated. We can see this clearly enough if

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we follow a line of argument due to von Weiszäcker. Nature, he says, and the concept of Nature, are the product of human creative minds (so far Dingle would agree) ; but our concept of Nature shows that these very minds, being human minds, are themselves an element of a developing Nature. Otherwise history and much biology, including the theory of evolution, must be abandoned. Man is a child of Nature, Nature is a construct of man. There is a closed circle here, of which the first part needs to be held in mind no less than the second.

In the next place, I see no grounds for supposing that all knowledge is rational knowledge. The religious man—not by any means necessarily Christian—who says that his religion has the indubitable element of personal experience as part of its basis (so far Dingle would agree) but insists that it has a further element, labelled for convenience as Revelation, may be contributing something even to science and scientific understanding. (The word Revelation is here being used, not in the technical sense of ‘special revelation’, as for example to the prophets of the Old Testament, but in the sense of ‘personal revelation’, of the feeling of given-ness, which religious people treat as one of the most significant characteristics of their faith.) Sir Lawrence Bragg, in a lecture appropriately called ‘Science and the Adventure of Living’, puts this in its scientific setting : ‘When one has sought long for the clue to a secret of nature, and is rewarded by grasping some part of the answer, it comes as a blinding flash of revelation ; it comes as something new, more simple and at the same time more aesthetically satisfying than anything one could have created in one’s own mind. This conviction is of something revealed, and not something imagined.’ I think Dingle sees—or perhaps, better, feels—this, for he can write of reason : ‘We may describe it at leisure, because it is always there, but when we have done we shall find that we have described something else’.

My third, and last, reaction concerns the limitations to be placed upon the denial of an objective Nature. Most of these arguments have been well enough known since the days of *cogito ergo sum*, and certainly Professor Dingle knows them better than I. But it is wise, none the less, to have them in mind. If there is no real particle to have position and momentum, but only a mental concept, what grounds have we for a real ‘you’ with whom I speak and have fellowship ? How can I avoid that solipsism which seems to denigrate life of a large part of what, as it would now appear, mistakenly, I treasure ? For if you, and all my neighbours, are just concepts of my mind, introduced to rationalise my experiences at the breakfast table and the golf club (themselves just concepts of my mind introduced to rationalise . . .), I am in danger of slipping into a state against which something within me rebels. Such rebellion, in whatever form it comes, is as much an experience as the observation of a star. There

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is something, of some sort, somewhere ; we do not live, alone, in the Land of Let's-Pretendia. And a man, reflecting on his own being which demands to be explained ; on reason, which cannot be described ; on revelation so persistently intruding even into scientific effort ; and on the sense of purpose and choice behind even a glance into the scientific sky, may well be tempted to assert that there is ultimate coherence only in some wider concept, to which he will give the name of God. To do so is not to take away from anything that others have done ; rather is it to put it in its rightful place.

These comments are not meant to undermine the book itself. I am sure that I shall not be the only one who will read this account with enormous pleasure, and look forward to its fulfilment in the later book whose possible birth the author envisages in his preface to this one.

C. A. COULSON

Problems of Life—An Evaluation of Modern Biological Thought, Ludwig von Bertalanffy (C. A. Watts & Co., London, 1952. Pp. ix + 216. 25s.)

THOSE who are familiar with Professor von Bertalanffy's writings will welcome a translation of *Das Biologische Weltbild*. This book contains a synthesis of the many papers which the author has written since 1926 and a very stimulating synthesis it is. Bertalanffy has a wide knowledge of biology and thinks about the problems he finds. As a result of his thinking he has admirably expounded the major problems and bravely suggests a possible study which he hopes may solve most of the difficulties.

He starts by pointing out that the traditional way of treating biological problems is by taking either a mechanistic attitude or a vitalistic one towards them. The latter is scientifically unsound because it renders the problem unassailable by experiment. The former is inadequate because, being based on an interpretation of all processes in terms of their structural conditions, it is unable to deal with such phenomena as regulation. Later on in the book an interesting comparison is drawn between the history of physics and the history of biology. We are told that the trend in physics is now towards an explanation of microphysical units in terms of the system to which they belong. Biologists have felt the need for a similar type of explanation, but resorted to vitalism. Over the past twenty years Bertalanffy has been suggesting that this 'holistic' attitude can be expressed in biology by the 'organismic conception' which is

The conception of the system as a whole as opposed to the analytical and summative points of view ; the dynamic conception as opposed to the static and machine-theoretical conceptions ; the consideration of the organism as a primary activity as opposed to the conception of its primary reactivity.

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In a more detailed discussion of living things it is noticed that organisms have three principle attributes : (a) some sort of organisation, (b) a dynamic flow of processes, and (c) a historical aspect.

Considering each in turn some interesting ideas are expressed. First, it is a general principle of organisation that it exhibits hierarchical order. Several examples of this are given ; division hierarchies, spatial hierarchies, genetic hierarchies and so forth. Unfortunately, the remarks in each case are somewhat intuitive and are not clearly enough worked out to be immediately useful. For example, mention is made of the analysis of the notion of a hierarchy by Woodger, but the account given is that of the latter author's first paper on the subject. No attempt is made to give the more exact formulation given in Woodger's *Axiomatic Method in Biology* (1937). From this it could have been seen that a relation R which generates a hierarchy has to have certain definite properties. It must be, for example, a one-many relation. From the definition, theorems about the relation can be deduced. Thus, the statements which can be made about hierarchies if they are to come from Woodger's definition will have the precision which is in it. Bertalanffy does not appear to have realised that this definition is precise and that some of his examples of hierarchies do not come, as they stand, under the clear definition which he implies he wishes to take as a starting point. For example, the spatial hierarchy into which an organism can be divided requires the recognition of sets of parts at the various levels in the hierarchy which must be characterised quite independently of the generating relation for the hierarchy. This characterisation is the part of the process of describing such a hierarchy which requires logical clarification, and the author does not seem to realise this. In other words, the hierarchical concept is extremely vague on the intuitive level, and by remaining on that level Bertalanffy has contributed little to its general clarification.

It is the second attribute of living things to which most attention has been paid and in which a possible positive approach has been made. A living system, thermodynamically, is an open system, maintaining itself in a steady state. This steady state is defined by the approach of minimal entropy production. A revolutionary consequence of this appears to be that 'in the transition to a steady state within an open system there may be a decrease in entropy and a spontaneous transition to a state of higher heterogeneity and complexity'. Our attention is drawn to the contemporary study of open systems. From such a study it is hoped to make explicit in thermodynamic terms such phenomena as epigenesis, regulation, and the tendency for living systems to approach the same end from different starting points (equifinality).

The realisation of the historical aspect of living things gives a third series of interesting comments. In passing, the author gives the view of

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Radl on the influence of the Manchester school of economists on evolutionary hypotheses, showing how this led to a view of selection by which everything which is useful will be selected and that which is selected is useful. There are some cogent comments on selection; notably, that because selection represents a necessary condition of evolution, it is not also a sufficient condition.

Throughout the book an emphasis is placed on the search for some unitary concept which is applicable to the physical, biological and even the psychological and sociological sciences. An attempt is made to express this by 'General System Theory' which is said to be found in the later pages of the book. Unfortunately, what is said about such a theory is so programmatic as to be very unsatisfying, but we are promised a second volume in which this will appear. I think that most people who read this book will eagerly await such a second volume.

One aspect of the book which must commend itself is the good bibliography of work which has been done on the continent of Europe in theoretical biology, especially recently.

R. F. J. WITHERS

The Origin of Life and the Evolution of Living Things. An Environmental Theory, Olan R. Hyndman, M.D. (Philosophical Library, New York, 1952. Pp. xxii + 648. \$8.75)

THE 636 pages of this book embody the thoughts and ideas of a doctor of medicine on the fundamental problems of biology. We are told that the manuscript 'took form' over a period of four years; fortunately for the reader a General Summary and a 'brief outline of some major concepts in the theory' are also provided to indicate what this form is, but even so a considerable period of mental readjustment to the author's train of thought will probably be needed before the ideas can be assimilated by anyone used to the customary scientific meanings of words in the English language.

The author informs us that he purposely avoided reading a great deal of the philosophies of others before he had formulated his own. The text and the bibliography show that he has not remedied this deficiency. His starting point seems to be a faith in the existence of truth, even if relative, and a belief in causation. Evolution by 'mutation' and 'natural selection' is inconsistent with this belief; environment determines differentiation and adaptation; 'the mechanism is achieved by accrued genetic organisation', 'co-genecity' of soma and germ cells being ensured through the agency of 'reacthins' which are 'swept to every cell in the organism'. I. V. Mitchurin and T. V. Lysenko are some of the people of whom the author

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became aware after completing his book, but he recognises that in respect to these fundamental concepts his theory is in accord with that of Mitchurin.

The main part of the work is taken up with a discussion of 'orders of differentiation', reminiscent of the ideas of Woodger and L. von Bertalanffy. The First Order of Differentiation is the 'germ integer' which 'catalyses oxidation by utilising some of the resulting energy in its own resynthesis'. The Second Order is the cell 'in which germ integers integrate to a common unit', the cytoplasm being 'an interphase between chromatin and environment' providing conditions for 'accrued organisation of germ substance' to correct any incongruity due to environmental change. The Third Order of Differentiation is that in which the cell unit is integrated to a larger common unit, this order being ushered in by the 'grand mutation' of diploidy, not accidentally, of course, but 'in response to the potential for differentiation'. The orders are analogous and parallel phenomena can be found in each of them, the most important being 'retracement' in their mode of reproduction; resynthesis of the integer, the cell and the multicellular form occur in the chronological order in which they were evolved separately. Mitotic division and sexual reproduction, which are discussed in general terms, illustrate this principle for the second and third order respectively. 'The integer must always retrace.'

It is alarming to find a reputable American publisher issuing this stuff.

J. W. S. PRINGLE

Mind—A Social Phenomenon, F. S. A. Doran (C. A. Watts & Co., London, 1952. Pp. 182. 10s. 6d.)

THE aim of this book is to strengthen 'the materialistic hypothesis' that mind is a function of cerebral activity. The author seeks to do this by taking seriously the 'blotting paper' theory of the mind, and by stressing the social control and conditioning of ideas in science and especially in medicine. Accordingly, most of the book consists in compressed historical surveys of the development of certain ideas and certain types of mind, such as 'the medical mind'. The conclusion drawn is that the beliefs which a physician, for instance, holds at any time are not his spontaneous creation, but are borrowed from contemporary non-medical thought which is adapted to meet the special case of human ailments. The only exceptions allowed by Mr Doran to this almost-universal 'borrowing' are the outstanding creative thinkers, 'the few' to whom 'the many' are permanently indebted: and even these have to accommodate their thinking to the traditions of their society.

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Mr Doran evidently believes that recognition of the social control of thought is a step towards the attainment of a physiological psychology. His argument seems to be that this recognition makes possible the explanation of most human thinking in terms of sensory perception ; Hume's views are mentioned approvingly. All social learning and communication can ultimately be traced back to perception, which admits of a physiological explanation. This seems to me a considerable over-simplification of the many unsolved problems in this field. In any case it seems clear that communication implies a transmitter with something to transmit, and somebody sometimes has to originate something fresh to communicate if communication is to be of much value. Whether this happens seldom or frequently does not raise any fundamental question of principle for the project of unifying psychology and physiology, and I cannot regard this book as furthering the project appreciably.

R. J. SPILSBURY

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

- W. E. Agar, *A Contribution to the Theory of the Living Organism*, first published 1943, 2nd edition (revised) 1951, Melbourne University Press (London : Cambridge University Press), pp. vi + 235, 27s. 6d.
- Franz Alexander, *Psychosomatic Medicine—Its Principles and Applications*, George Allen & Unwin, Ltd., London, 1952, pp. 300, 21s.
- John R. Baker, *Abraham Trembley, Scientist and Philosopher, 1710–1784*, Edward Arnold & Co., London, 1952, pp. xix + 259, 35s.
- George Boole, *Studies in Logic and Probability*, Watts & Co., Ltd., London, 1952, pp. 500, 25s.
- A. C. Crombie, *Augustine to Galileo. The History of Science A.D. 400–1650*, Falcon Educational Books, London, 1952, pp. xv + 436, 42s.
- Jan Dembowski, *Science in New Poland*, Lawrence & Wishart, Ltd., London, 1952, pp. 59, 2s. 6d.
- R. D. Gray, *Goethe the Alchemist*, Cambridge University Press, 1952, pp. x + 311, 35s.
- F. A. Hayek, *The Counter-Revolution of Science*, The Free Press, Glencoe, Illinois (London : George Allen & Unwin, Ltd.), 1952, pp. 255, 32s.
- T. E. Jessop, *Berkeley : Philosophical Writings*, Thomas Nelson & Sons, Ltd., Edinburgh, 1952, pp. xxv + 278, 10s. 6d.
- Martin Johnson, *Time and Universe for the Scientific Conscience* (Sixth Arthur Stanley Eddington Memorial Lecture), Cambridge University Press, 1952, v + 42, 2s. 6d.
- C. Hillis Kaiser, *An Essay on Method*, Rutgers University Press, New Brunswick, New Jersey, 1952, vi + 163, \$3.25.
- Jürgen v. Kempfki, *Charles S. Peirce und der Pragmatismus*, W. Kohlhammer Verlag, Stuttgart und Köln, 1952, pp. 115.
- Fred Kohler, *Evolution and Human Destiny*, Philosophical Library, New York, 1952, pp. 120, \$2.75.
- R. W. Meyer, *Leibnitz and the Seventeenth-Century Revolution* (translated by J. P. Stern), Bowes & Bowes, Cambridge, 1952, pp. 227, 25s.
- The late E. A. Milne, *Sir James Jeans*, Cambridge University Press, 1952, xvi + 176, 21s.
- E. A. Moody and Marshall Clagett, *The Medieval Science of Weights*, University of Wisconsin Press, U.S.A., 1952, pp. x + 438, \$5.00.

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- O. Neugebauer, *The Exact Sciences in Antiquity*, Princeton University Press (London : Geoffrey Cumberlege), 1952, ix + 191, 42s.
- D. J. O'Connor, *John Locke*, Penguin Books, Harmondsworth, Middlesex, 1952, pp. 222, 2s. 6d.
- J. A. Passmore, *Hume's Intentions*, Cambridge University Press, London, 1952, pp. ix + 164, 18s.
- Ian T. Ramsey, *Miracles : an Exercise in Logical Mapwork* (an inaugural lecture), Clarendon Press, Oxford, 1952, pp. 24, 2s. 6d.
- Bernhard Rensch, *Psychische Komponenten der Sinnesorgane*, Georg Thieme Verlag, Stuttgart, 1952, pp. 200, DM22.50.
- Stephen Toulmin, *The Philosophy of Science*, Hutchinson's University Library, London, 1953, pp. viii + 176, 8s. 6d.
- Florian Znaniecki, *Modern Nationalities*, University of Illinois Press, Urbana, Illinois, 1952, pp. xvi + 196, \$3.95.

(b) JOURNALS

The contents of the latest issues of MIND and of the JOURNAL OF THE HISTORY OF IDEAS are printed with the advertisements on the cover of this issue

Contributions to the Analysis and Synthesis of Knowledge (Proceedings of the American Academy of Arts and Sciences, May, 1952, 80), No. 2

Philipp Frank, 'The origin of the separation between science and philosophy'
 Bernard Barber and Robert K. Merton, 'Brief bibliography for the sociology of science'

Alonzo Church, 'Brief bibliography of formal logic'

Laura Thompson, 'Some significant trends towards integration in the sciences of man'

Dialectica (Zürich, Switzerland), 1952, 6, No. 2

F. Gonseth, Editorial : 'Le principe de dualité'

F. Gonseth, 'La loi du dialogue'

S. Gagnebin, 'Théorie et expérience. Le problème'

P. Bernays, 'Dritte Gespräche von Zürich. Ansprache'

P. Bernays, 'Gesichtspunkte der Erkenntnistheorie (insbesondere zur Auseinandersetzung mit der Kantischen Erkenntnistheorie)'

W. Pauli, 'Theorie und Experiment'

F. Gonseth, 'Théorie et expérience'

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S. Gagnebin

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L. Husson

A. Mercier

S. Moser

Ch. Perelman

} 'Discussion préliminaire'

ON THE PHILOSOPHY OF SCIENCE

P. Bernays, 'Bemerkungen zu der Arbeit von E. E. Harris'

E. E. Harris, 'Some recent criticisms of Berkeley'

E. u. H. Guggenheimer, 'Technische Epistemologie'

Philosophy of Science, October, 1952, **19**, No. 4

A. Bachem, 'Heisenberg's Indeterminacy Principle and life'

Howard Becker, 'Science, culture, and society'

Adolf Grünbaum, 'A consistent conception of the extended linear continuum as an aggregate of unextended elements'

Charles F. Hockett, 'An approach to the quantification of semantic noise'

John G. Kemeny and Paul Oppenheim, 'Degree of factual support'

Edward H. Madden and Marian C. Madden, 'Chauncey Wright and the logic of psychology'

Henry Margenau, 'Physics and ontology'

Omar Khayyam Moore, 'Nominal definitions of "Culture"'

George K. Plochmann, 'Professor Henle on the four figures of the syllogism'

(c) ARTICLES

N. Bohr, 'Hendrik Anthony Kramers', *Ned. T. Natuurk*, **18**, 1952, 161

A. C. Crombie, 'The invention of scientific explanation', *Discovery*, 1952, **13**, 345

'The invention of the experimental method', *ibid.*, 1952, **13**, 391

'The 17th-century revolution in mathematical physics', *ibid.*, 1953, **14**, 23

Adolf Grünbaum, 'Causality and the science of human behaviour', *American Scientist*, 1952, **40**, No. 4, 665

Germán Fernández Guizzetti, 'Los datos de la lingüística y el carácter evolutivo de la razón', *Sociedad Argentina de Filosofía*, 1951

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C. F. A. Pantin, 'Darwin's theory and the causes of its acceptance', *The School Science Review*, 1950, No. 116; 1951, No. 117; 1951, No. 118

Raymundo Pardo, 'El carácter evolutivo de la razón y dos ciencias tipo: la física y la lingüística', *Sociedad Argentina de Filosofía*, 1951

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G. Polya, 'On plausible reasoning', *Proceedings of the International Congress of Mathematicians*, 1950

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- Pierre A. Radwanski-Szinagel, 'Problèmes des synthèses de la science de l'homme', 1949, *Bulletin de la Société royale belge d'Anthropologie et de Préhistoire*, 32-44
- Roberto Remuñán, 'Los datos de la biología y de la sociología (etnopsicología) y el carácter evolutivo de la razón', *Sociedad Argentina de Filosofía*, 1951
- J. O. Wisdom, R. J. Spilsbury, D. M. MacKay, Symposium on 'Mentality in Machines', *Proceedings of the Aristotelian Society*, London, 1952, Supplementary Volume 26, 1-86

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